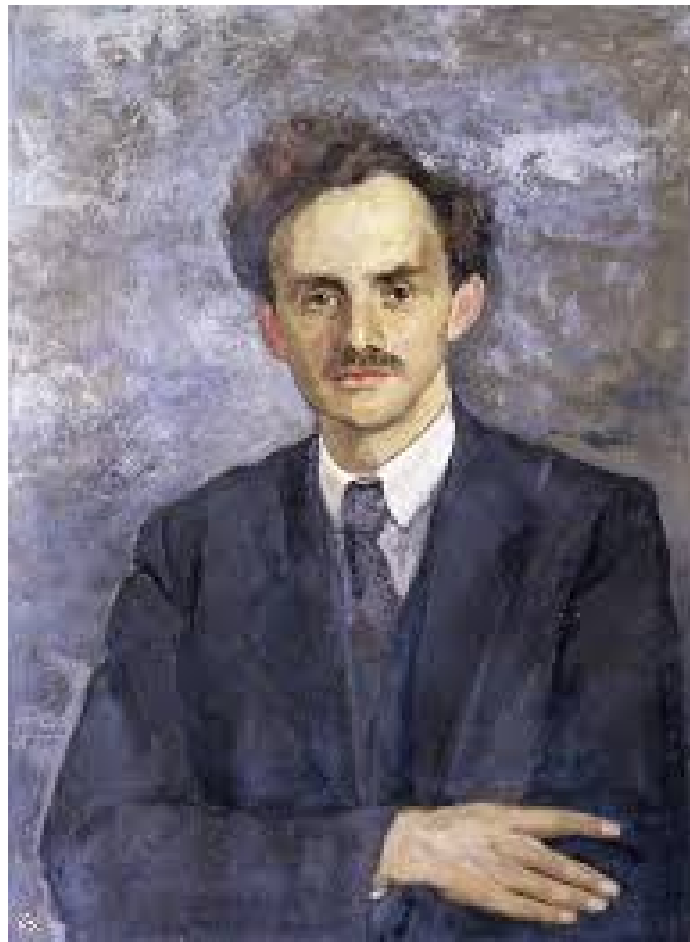


**Interviews
with
Prof. P.A.M. Dirac**



Compiled by Symmetry Seeker

Credits

This work has been compiled and edited to publish using the source material available on the official website of American Institute of Physics.

I do not own any part of this work. All credits to American Institute of Physics.

This work is strictly meant for non-commercial uses only.

Title page picture credit : American Institute of Physics.

Contents

Session Number	Date of Interview	Interviewed by
1	1st Apr 1962	Thomas S. Kuhn & Eugene Wigner
2	6 th May 1963	Thomas S. Kuhn
3	7 th May 1963	Thomas S. Kuhn
4	10 th May 1963	Thomas S. Kuhn
5	14 th May 1963	Thomas S. Kuhn

Interview Session - 1

Dirac:

I started my education at Bristol. It was my home town. My father was a master in one of the schools there. He taught French. I went to the same school. When I completed that I moved on to the faculty of engineering of the University of Bristol. It was in the same building. So I continued my work in the same building. I got pushed on pretty fast because the First World War was on at the time. That meant that the young men were called up and there were plenty of vacancies. I started the University when I was 16. I got a degree in electrical engineering at 19. I looked around for an engineering job, but we were just at the depression at that time and I couldn't get any. My professor of engineering said that instead of just hanging around doing nothing, I should do some research in engineering. I had just started that for a few weeks, when he suggested that I take a course in mathematics at the University of Bristol. That involved going to a different building.

Kuhn:

How much mathematics had been in the engineering curriculum?

Dirac:

Differential equations — as far as that. I mean they were not concerned With mathematical rigor; they were concerned with getting results.

Kuhn:

Were transient circuit techniques — this sort of thing — involved?

Dirac:

Yes. I think that this engineering education has influenced me very much in making me learn to tolerate approximations. My natural feelings were to think that only an exact theory would be worth considering. Now, engineers always have to make approximations. I learned that even a theory based on approximations could be a beautiful theory. I rather got to the idea that everything in nature was only approximate, and that one had to be satisfied with approximations, and that science would

develop through getting continually more and more accurate approximations, but would never attain complete exactness. I got that point of view through my engineering training, which I think has influenced me very much. As a result of that I haven't been much interested in questions of mathematical logic or any attempts to form an absolute measure of accuracy, an absolute standard of reasoning. I feel that these things are just not important, that the study of nature through getting ever, improving approximations is the profitable line of procedure.

Kuhn:

Had you been heading for engineering right along?

Dirac:

I think so. I had an elder brother who also took engineering, and I tended to copy him. I didn't have much initiative on my own. That path was rather well set out for me, and I didn't know very well what I wanted.... The man I was in closest contact with in my engineering work was Professor David Robertson, who was a professor of electrical engineering. I was very much influenced by him. I

was very much impressed by the need to have foresight to avoid accidents. He also arranged things to show the mathematical beauty, I might say, of many of the calculations that one had to make. He was a man who was paralyzed through polio, I think. He moved around in a wheelchair everywhere. He had to organize his whole life to be able to manage with that handicap. He seemed to do it pretty well. I then moved over to the mathematic department at Bristol University and came under the influence of Professor Hasse, the professor of applied mathematics was, and Mr. Fraser, a lecturer in pure mathematics. Mr. Fraser was an extremely good lecturer. He introduced me to the ideas of mathematical rigor, and he was able to make the subject interesting. I spent two years on his course in mathematics at Bristol University, then I went to Cambridge as a research student. By going to Cambridge as a research student, I didn't need to learn Latin. Latin was a compulsory subject at Cambridge, but they never taught it at my school. I should say that the school I went to as a secondary school was an extremely good scientific school. Most of my time there was spent learning science and mathematics and modern languages..... At an early stage in

secondary school I was allowed to do mathematical reading by myself at a more advanced level than the rest of the class. I was just told which books to read and I studied them by myself.

Kuhn:

What sort of books did you read?

Dirac:

Books on differential and integral calculus. I think Edwards was the author, but I'm not very sure. I don't think it matters very much which books one reads, if one has reasonably good books.

Kuhn:

Did you get on to other parts of mathematics at that point, besides calculus?

Dirac:

Just geometry and algebra.... I think all the time I picked up my mathematics more by working by myself than from lectures. I don't seem to be able to pick things up very much from a lecture because I like to jump forward and jump back again and jump forward and back, continually. One can't very well

do that if one is listening to an ordered presentation like a lecture. When I go to lectures I usually just get stimulated to think on certain lines, and then maybe I think along those lines myself instead of listening to every word the lecturer says. I perhaps miss a good deal of the lecture for that reason and have to make it up later in my own reading or something. But all my learning in mathematics has been rather along those lines. And it still is like that.

Kuhn:

To what extent did books outside of the curriculum also have this same role of starting problems, starting ideas?

Dirac:

I didn't have very many books available to me. I just had a few which I studied thoroughly.... I know I was always very interested in the fundamental problems of nature. I would spend much time just thinking about them. I remember I thought out for myself that there might be a connection between space and time coordinates such that when one changes one's time axis, one would also rotate one's space axis. But I knew nothing about the hyperbolic

geometry at that time. I could see that it wouldn't work just with Euclidian space because going through big angles, you coon get into contradictions.

Kuhn:

How far did the science itself go in school? Were you again there encouraged to go further than the courses?

Dirac:

To some extent, but not as much as I would have liked to. I had the advantage that I was pushed up into a higher class than would normally correspond to my age just on account of the war. All the older students were sent off to war work and left the higher classes empty. That meant that all through my secondary schooling I got put into a higher class than would correspond to my age. It was an advantage from the point of view of learning things early.

Kuhn:

When 'you did the two years of math at Bristol, what subjects did you then have to deal with?

Dirac:

I learned about the rigorous setting up of calculus. Previously, in the engineering faculty, one just learned it from the practical point of view, and one didn't bother about getting things rigorous. I also learned differential geometry. That was Fraser. I found that a most interesting subject. Did I say differential geometry? I made a mistake, I should have said projective geometry. No, I did not do differential geometry at that stage. Projective geometry I found a most fascinating subject. One could get quite powerful results — theorems about straight lines and conies intersecting each other — just from elementary arguments about 1 to 1 correspondence. That appealed to me very much. All my work since then has been very much of a geometrical nature, rather than of an algebraic nature.

Kuhn:

Did you do abstract algebra?

Dirac:

No, I don't know that that existed then. Did it? I did not do any of that. I learned a little of quaternion just by reading up on it myself. I got hold of Thomson's textbook. Rather heavy- going. There was quite a lot of unnecessary heaviness in the beginning of that book, but I learned something about it.

Kuhn:

Were quaternion still a live method in British education, or were there people using them still at Cambridge or at Bristol?

Dirac:

At Bristol, no. At least so far as I know. I can't remember how I first got hold of that interest myself. And not very much in Cambridge. But it is a subject that I find fascinating myself. I don't think it is very much appreciated. I don't think it is in general appreciated as much as it should be. * * *

Kuhn:

Did. you pursue the quaternion at all?

Dirac:

No, I didn't. When the war ended, there was tremendous interest in relativity. Previously we just hadn't heard about it at all, and then there was this tremendous interest. Professor Broad, who is a professor of philosophy, gave a course of lectures on the subject, which I attended.... Broad was at Bristol. He talked about it largely from the point of view of a philosophy. I tried to appreciate it, but I did not get very much success in trying to appreciate philosophy. But I did learn from Broad the three plus signs and one one minus sign, which was the basis of the Einstein theory. I saw that it was really something new which I had never thought of in my speculations about relations between space and time

Kuhn:

But it tied to those speculations.

Dirac:

Well, it provided a way out of the difficulty. The difficulty that one got into contradictions when one made too big rotations....

Kuhn:

There had been little or no talk of relativity in your scientific education up to that point?

Dirac:

Not even the special theory. I hadn't heard about it.

Wigner:

How much did you see personally of teachers in those places? at is something I always wanted to ask you.

Dirac:

I went to lectures. Do you mean, about social contacts?

Wigner:

Not only social.

Dirac:

I think once or twice they invited me to their homes. I know David Robertson invited me to his home a few times. Fraser also has invited me to his home.

Fraser wasn't married at that time. He married only later.

Wigner:

But did you see them personally for scientific discussions?

Dirac:

No. They did not have anything in Bristol corresponding to the supervision. Nor did the undergraduates in Cambridge. In the mathematics course the class was very small, it was only two people, a girl and, myself. If I remember — I can't at the moment. It may come back.

Wigner:

Did you talk to her a great deal about mathematics, about the subject?

Dirac:

No, I think we only met at the lectures and, separated afterwards, so far as I know.

Wigner:

You didn't invite her to tea?

Dirac:

Oh no. In fact I had no social life at all as a child.

Wigner:

You had no social life. Where did you stay, Paul?
 1 There did you eat, where did you sleep?

Dirac:

Well I lived with my parents, but nobody ever, came to our house, except a few pupils of my father who came from lectures. No one ever came for social purposes.

Wigner:

How much did you talk to your parents?

Dirac:

Very little. My father made the rule that I should only talk to him in French. He thought it would be good for me to learn French in that way. Since I found that I couldn't express myself in French, it was better for me to stay silent than to talk in English. So I became very silent at that time- - that started very early My father and mother usual ate

separately. My mother in the kitchen and father in the dining room I used. to eat in the dining room with my father; and. my brother and sister would eat in the kitchen with mother. That was a general rule. I think I would have preferred to be with the others, but there weren't enough chairs in the kitchen, and so I would eat in the dining room with my father.

Kuhn:

Were they younger or older than you?

Dirac:

My brother was older, and sister was younger.

Kuhn:

Was he very much older?

Dirac:

He was two years older. He committed suicide at the age of 24, which was a great shock to my family, of course

Wigner:

You've never told, me what the cause of it was, if you knew.

Dirac:

I suppose he was just very depressed. That kind. of life, brought up without any social contacts must have been very depressing to him as it was to me. And having .a younger brother who was brighter than he was must have depressed him also quite a lot.

Wigner:

Was it' evident that you were brighter than he was?

Dirac:

Yes it was. For instance, I got a first class in my engineering degree and he got a third class.

Wigner:

Did he have a Job?

Dirac:

Yes, he got an engineering job in the Midlands, Coventry, for some time, and Wolver Hampton. I think be seemed. to like to come home, even though there wasn't much social life at home. The small amount of vacation that he would get he would

immediately spend the time by cycling to Bristol and cycling back again in the end, spending as much time at home as he could.

Wigner:

Was he interested in any girls?

Dirac:

He did have a' girlfriend, yes, but I don't know how close his relationships with her were.

Wigner:

What did she say when he died?

Dirac:

I don't know. 'My father did suggest that this girl should come and visit us, and my mother said, "Oh no, she mustn't, she might go after Paul." I was 22 years at that time, but my mother still thought I needed to be protected from girls. I rather resented it, but the result was that I never met her. There was some mystery connected with my brother's death. He left his job three months before he died, and what he did during those last three months, no one was able to find out. He didn't tell his landlady that he'd.

left his job. He continued to leave regularly in the morning and come home in the evening. His landlady did not know that he had left his job. He continued to pay his rent regularly. He just withdrew his savings, and when they were gone he killed himself. The police made extensive inquiries, but they were unable to find out what he did during those last three months.

Wigner:

Did he leave his job of his own ... ?

Dirac:

Yes, he left it voluntarily.

Wigner:

Did you know that he left his job?

Dirac:

Oh no. We didn't correspond with each other. In fact we didn't talk to each other for a number of years.

Wigner:

Why not, Paul?

Dirac:

Well, for one thing we had to talk in French or we'd get into a row. That was one reason for not talking.

Wigner:

This French rule applied to your brother also?

Dirac:

Yes, and to my sister. He thought he would make us learn French in that way. My father was Swiss by birth... My mother was English. I think father also had an unhappy childhood. He ran away from home without telling his parents and went to England.. He got married without telling them. His parents never knew that he was married until some years later when he visited them with a couple of children. That's what I heard from my mother.

Kuhn:

Did they encourage your turning to science from engineering?

Dirac:

My father, yes, always encouraged me toward mathematics. He appreciated very much the importance of a good education, and he always encouraged me in that way. He did not appreciate the need for social contacts. The result was that in those days I didn't speak to anybody unless I was spoken to. I was very much an introvert, and I spent my time thinking about problems in nature. And, of course, when I first learned about relativity, that was a period of great excitement.

Kuhn:

You went right on with it from there?

Dirac:

Well, I got Eddington's book and studied that for some time

Wigner:

How does it come that Eddington had a book ready so soon if relativity was not known at all in England?

Dirac:

Well it wasn't known at Bristol. Eddington did get off very quickly. Eddington introduced the subject to England.

Kuhn:

These lectures of Broad's would have been immediately after the war?

Dirac:

I should think: about 1918. I'm not too sure of the exact date.

Kuhn:

This would have been before the eclipse expedition?

Dirac:

Yes, and of course the eclipse expedition did enhance that interest.

Kuhn:

Were there other things about relativity that you recollect as being particularly exciting?

Dirac:

Well the idea of curved space was introduced at that time also. We learned about the special theory and the general theory of relativity simultaneously.

Wigner:

Did the constancy of the velocity of light play any role in your way of thinking?

Dirac:

I didn't know that it was constant.

Kuhn:

You got the relativity in a fairly highly mathematical formulation at the time you first heard of it.

Dirac:

No, Broad didn't do very much mathematics. He didn't prepare me.... It needed some time to get used to Eddington, to get used to the tensor calculus.

Kuhn:

Were there people working hard at it, discussing it at all, at Bristol then? I mean was this a general excitement, or was this something unique?

Dirac:

Other students were. Just one of those early students with me I've kept in touch with — a man called Wilshire. I believe we used to talk about it together. He is living not far from Cambridge and visited me a few years back. You could get more information from him about it.

Kuhn:

What is his first name?

Dirac:

I think it is H. C., but I am not sure about that. He works at an aeronautical college near Bedford.

Kuhn:

What was your sense of change when you went from Bristol to Cambridge? Did you find the environment very different, the subjects more advanced?

Dirac:

Yes. First of all going away from home. I had been away from home previously during a long vacation. During my engineering course I went to (Rugby) for about a month or so to get some practical experience in engineering work at the B. T. H. works - - the British Thompson Houston Works in Rugby. My brother was also working there at the time, but we never met. If we passed each other in the street, we didn't exchange a word. I got some practical experience there working in an engineering factory. I did not please my employers very much, and they sent an unfavorable report back to my professor, Professor David Robertson.

Wigner:

How did you find out about this?

Dirac:

He told me. David Robertson, yes, he showed me the report.

Wigner:

What did the report say-?

Dirac:

It said I lacked keenness, and was slovenly.

Wigner:

You lacked keenness? And you are slovenly?

Dirac:

Yes. Yes. Those were the crucial things that they said.

Wigner:

Not that you are uncommunicative?

Dirac:

No. Why should a factory be concerned whether one is communicative or not?

Kuhn:

What sort of work were you actually doing there?

Dirac:

Well, turning things on lathes and metal work.
Filing, drilling.

Kuhn:

Bad you had any manual experience of that sort before?

Dirac:

Yes, I had had some in school. This Merchant Venturer School where I went was in the same building as a technical college. It was used as a school in the daytime and a technical college in the evening. It had all the equipment for a technical college, and it was at the disposal of the school. So I actually had metal-working. [Interruption] In Cambridge I came under the influence of R. H. Fowler.... He was my supervisor. Every research student has a supervisor. I came really into closer contact with him than with my Bristol professors, mainly because of the supervision system.... In Cambridge theoretical physics belongs to the mathematics faculty. I was attached to the mathematics faculty. One of the questions which I've always wanted to ask you, Paul, is when did you make a transition from relativity theory to quantum theory?

Dirac:

Well, when I went to Cambridge, I learned about the Bohr theory of the atom from Fowler. I had no idea that atomic theory was so developed. It came as a surprise to me. I had not heard of the Bohr theory of the atom at all in Bristol.

Kuhn:

Had the problem of the quantum arisen there at all? Do you remember?...

Dirac:

I don't think I'd heard about that at all. I was in a mathematics section in Bristol, not in the physics section. They were in different buildings. I didn't have any contact with the physics people.

Kuhn:

Theoretical physics then was not really related with mathematics at Bristol?

Dirac:

At Bristol there was just mathematics, which was divided into the pure and applied parts. I did the

applied part. I remember a student, Miss Dent was her name. The first year of the two mathematics years at Bristol we did both pure and applied. The second year I had to choose between them. I wasn't very definite whether I wanted to choose either pure or applied, but Miss Dent definitely wanted to do applied. The professors of course wanted us both to do the same. Otherwise they would have had to give twice as many courses. So by that accident I got shifted into the applied field.

Kuhn:

But the applied mathematics did not include as much theoretical physics as it did at Cambridge?

Dirac:

No, it did not include anything about the Bohr theory of the atom. It was largely about potential theory and solutions of equations, namely $V = 0$.

Wigner:

How did Fowler get you interested in atomic physics and in quantum theory?

Dirac:

It was his subject.

Wigner:

He talked to you?

Dirac:

Yes, he talked to me about it.

Wigner:

How did he talk? Were you alone when you talked?

Dirac:

I had talks with him alone and also went to his lectures. And, in the lectures I learned about Hamilton methods in dynamics.

Kuhn:

These had also not been part of the applied math?

Dirac:

To some extent I learned about them at Bristol, but I hadn't gone very far with them. It was in Cambridge

that I saw the need for the Hamiltonian methods, to connect with the Bohr- Sommerfeld quantization.

Kuhn:

These would have included transformation theories? Hamilton-Jacoby equations?

Dirac:

Yes. Yes. I learned that partly by myself from Whitaker's *Analytical Dynamics*.... Also Sommerfeld's book. In the appendices, he gives the Hamilton - Jacoby theory.

Wigner:

Haas' book is the one from which I learned it — *Theoretical Physics*.

Dirac:

I didn't' learn it very thoroughly because I remember it was on a Sunday that the idea first occurred to me that $ab - ba$ might correspond. to a Poisson bracket. But at that time I didn't' know exactly what a Poisson bracket was, so I wasn't able to check whether it was right. I didn't have any book at home which dealt with Poisson brackets. I had to

wait until the next Monday and go to the library and look up Poisson brackets there and check to see if it was right.

Kuhn:

The ab- ba notion and the relation to Poisson brackets came when in relation to Heisenberg's trip to Cambridge?

Dirac:

Heisenberg made his trip to Cambridge, in I think, June, or it might have been July, of 192. He gave a talk about a new theory to the Kapitza Club, but I wasn't a member of the club so I did not go to the talk. I did not know about it at the time. The first I heard of it was in September when Fowler sent to me a copy of the proofs of Heisenberg's paper and asked me what I thought about it. That was the first that I heard about it. I think Fowler found it interesting. He was a bit uncertain about it and wanted to know what my reaction to it would be. When I first read it I did not appreciate it. I thought there wasn't much in it and I put it aside for a week or so. Then I went back to it later, and suddenly it became clear to me that it was the real thing. And I

worked on it intensively starting from September 1925. I think it s just a matter of weeks or so before I got this idea of the Poisson brackets....

Wigner:

I don't think I ever had as extended a conversation with Paul as we are having now, at least not in What was your daily occupation? How much did you go to lectures, how much did you sit in your room, how much did you talk to people? Did you go to theatres?

Dirac:

I never went to theatres. I spent most of my time by myself, sitting working things out or going for walks. I used to spend every Sunday going for a long walk, a whole day walk, taking n lunch with me, like I did yesterday. During those long walks I would not intentionally think about my work, but I might perhaps review it. I found these occasions most profitable for new ideas coming. It was on one of those occasions that the possibility of ab-ba corresponding to a Poisson bracket occurred — on one of those Sunday walks.

Wigner:

But on week days, how much time did you spend in lectures, how, much in your room?

Dirac:

I don't remember just how many lectures I had. maybe four or five a week, something of that order. I might be able to look it up.... I have some notebooks of my lectures. But I would mainly spend the mornings and the evenings studying and took short walks in the afternoons. With a long walk all day Sundays.

Wigner:

Did you have any friends that you saw consistently?

Dirac:

The other research students in my college. I would meet them at dinner every evening.

Wigner:

But not other times too much?

Dirac:

Occasionally I'd be asked to tea, but not very much on that order.

Wigner:

Did you read any literature?

Dirac:

I think I read a little. I don't have any-thing outstanding in my mind of that type.

Wigner:

I have gaps in my knowledge of Paul, and I thought I'd try to fill these.

Kuhn:

I'm very glad you did. Did you also hear Fowler on statistical mechanics?

Dirac:

Yes. I learned about the Boltzmann equation.... Lennard-Jones was there at the time, and he was very much concerned with statistical mechanics.

Wigner:

How did statistical mechanics and quantum mechanics compare in your mind?

Dirac:

I didn't like the statistical mechanics quite so much.

Wigner:

Why not, Paul?

Dirac:

I suppose because of the approximations in it, and the complication of the Boltzmann equation. You have this collision term coming in, which means all the really important things are lumped together in one term, which is not explained very well. I disliked that very much. [Introduction of Mrs. Dirac]

Wigner:

This doesn't agree with the approach of Gibbs.

Dirac:

Yes, I like the Gibbs work very much.... One of my early papers was on statistical mechanics. Not the

Gibbs theory. It was a paper which I published in the Royal Society on conditions for statistical equilibrium between atoms, electrons, and radiation. This was before quantum mechanics came out.... My first in the Proceedings.

Kuhn:

That grew out of Fowler's lectures, Lennard-Jones?

Dirac:

Yes, detailed balancing was the subject. It was much talked about then. I wanted to get some information about atomic processes by using this principle of detailed balancing. The adiabatic hypothesis was another of the general principles which one used in those days. I wrote a paper on that.

Kuhn:

Did one generally, in getting into a subject of this sort, get really quite full familiarity with the continental literature also?

Dirac:

Fowler made a visit to Copenhagen, maybe more than one visit. And he'd come back and tell us about

what he'd heard in Copenhagen. He inspired in that way. It was through these visits of Fowler to Copenhagen that we were kept in close touch with what the world was doing.... I don't know that I read very much independently of what I got from Fowler. Oh yes, I think I did read *Zeitschrift für Physik* quite a lot. That was the main journal in those days. Yes, I read the ZS f. Physik quite a lot. I had learned German at school. Not fluently, but enough to be able to read scientific German at that time. Paul, one thing is more evident to me from your talk than it was before. Even in Bristol, they must have realized very clearly that you have an unusual imagination.

Dirac:

Yes, they did.

Wigner:

Now how did that come if they never saw you alone? Were they (7??) 7

Dirac:

Partly from examination results.

Kuhn:

Did you talk to any of them about some of your own thinking on problems of this nature?

Dirac:

No, no. I was very much an introvert. .I. still am, I expect.... I have heard that the mathematics department at Bristol was very disappointed when they first heard that I had decided to go in for engineering. When I finished my engineering course, they induced me to go study with them for a time. They gave me free tuition.

Wigner:

Whom do you mean by “they” in the mathematics department?

Dirac:

I suppose the professors there. Probably I got that from Fraser; I expect I got that information from Ivar. Fraser was the one that I knew best in the mathematics department at Bristol.

Wigner:

What do you mean by “knew best” if you talked to him so rarely?

Dirac:

I talked to the others still less • But he did invite me to his home once or twice.

Wigner:

How much older was he than you?

Dirac:

Quite a bit older I think, he must have been in his thirties when I was a student.... Everybody agreed that Fraser was a very good mathematics teacher. He didn't interest himself at all in research, but he was a very good teacher.

Kuhn:

You heard lectures of his while you were an engineer?

Dirac:

No, no.

Kuhn:

So your contact with him really came when you transferred to mathematics.

Wigner:

But by that time they must have already known or suspected that you were somewhat out of the ordinary?...

Dirac:

Yes, they must have heard about me previously, probably from examination results.... There was a matriculation examination which I first took when I was slightly less than 1 I took it a second time in more advanced subjects a year later. I'm not sure — I've got the dates wrong. I suppose I ought to check on that. But I did very well in the scientific subjects there.

Wigner:

Just the same there isn't the strength that you exhibit. Examination mainly requires competence.

Dirac:

Weil I don't know how they knew about me. I didn't worry about that.

Wigner:

How did your relation to people change in Cambridge when you first came out with your papers on Poisson brackets?

Dirac:

I don't think my relations to people changed at all. It was just that I became even more concentrated on my work. Oh, there was change in the sense that got elected to societies, the Kapitza Club, the Delta2V Club. I went to their meetings, gave talks to their meetings.

Kuhn:

Did. you find that giving talks on your own work came fairly easily?

Dirac:

Yes. In fact when you've just learned a subject yourself you are in the best position to be able to talk

about it because you still remember where the difficulties were. I think you can teach a subject better when you've just learned it than after a number of years. Do you find the same?

Wigner:

Yes, my best class in solid state physics was when I had no idea of the subject, and two of my students were Seitz and Bardeen.... I never found out if they realized that I knew the subject so poorly. I must ask them.

Kuhn:

Besides Fowler in your early days at Cambridge, were there other people with whom you had contact.

Dirac:

There was Cunningham, who taught classical electrodynamics. I went to his lectures. There was Milne who talked about interior of stars. There was Eddington. Eddington was a great man in those days. He introduced relativity to England.

Kuhn:

Was Milne at all concerned with relativity yet then?

Dirac:

I don't think he gave lectures on it, so far as I know.... Just at the moment I can't remember going to Eddington's lectures • Max Newman also gave a course on general relativity, geometrical aspects of the question.

Kuhn:

Were there any of these people whom you also talked with as you did with Fowler?

Dirac:

Not so much. Mime was my supervisor for a term while Fowler was absent in Copenhagen.

Wigner:

I thought Milne was hardly your contemporary.

Dirac:

He was a bit older, a little, probably five years older or so....

Kuhn:

There is an early paper on stellar constitution.

Dirac:

Yes, yes. That was done while I was under Milne's supervision. He suggested the problem.

Kuhn:

I take it from what you say that to a very great extent, what you got from places other than the nature of yourself came from lectures rather than from large amounts of reading.

Dirac:

Well, I think the lectures told me the directions in which I should look, and I fitted in the gaps from reading. As I said before, I would very- seldom hear everything that a lecturer said. Do most people listen to every word that a lecturer says?

Wigner:

I think it is very different if you know the subject reasonably well. Then you can follow every word. If you don't know the subject reasonably well, if it's entirely new, you don't appreciate every word. But didn't you say that you learned mechanics, Hamiltonian theory mainly, from books?

Dirac:

Fowler gave a course of lectures on it also.

Kuhn:

Did you go on with mathematics also at Cambridge, except to the extent that it was integrated into the rest of these subjects?

Dirac:

Only so far as I needed the mathematics for my research.

Kuhn:

Was there considerable excitement about the quantum and the problems of quantum mechanics at Cambridge in this period?

Dirac:

Yes, yes there was.

Kuhn:

Was there also that sense which again people speak of on the continent that something fundamental now had to come to get around these problems that were

just not responding. That there was something fundamentally the matter.

Dirac:

I am not sure that that is so. They had the Bohr-Sommerfeld method of quantization and they thought it would have to be extended in some way.... I don't think people suspected that one would need such a complete revolution... It rather came as a surprise to inc when Heisenberg's idea came out....

Wigner:

You know that it was not recognized on the continent that Heisenberg's paper was a fundamental departure. It was thought it might be an improvement of the Schwarzschild-Sommerfeld method of quantization. That it is a fundamental departure became clear to most of us very much later.

Kuhn:

When you say 'every much later", do you mean more later than the famous three man paper? V

Wigner:

About that time. But even then it was not recognized at all that it would bring the resolution of the fundamental difficulties. For instance the accumulation of energy in the photo-electron, the transfer of angular momentum to the atoms in the Stern-Gerlach experiment, and a number of other similar V basic phenomena. These were considered to be the basic fundamental difficulties. That these were solved was not recognized. It was understood in England, or were these difficulties not so much in the foreground?

Dirac:

I don't know how it was so much with people in general. I just know my own reaction. Just that I had been trying hard for two years to solve a certain problem without any success. Then I suddenly saw that Heisenberg's idea provided the key to the whole mystery.

Kuhn:

What was the problem?

Dirac:

To get a better quantum theory.... Which meant to be able to explain the helium atom.

Wigner:

You did not worry about problems such as how does the energy accumulate in the photo electron?

Dirac:

I tell my primary problem was how to explain the helium spectrum.

Wigner:

Did you read the papers connected with the explanation of the angular momentum transferred to the atom in the Stern-Gerlach experiment?

Dirac:

I remember hearing a lot about the Stern-Gerlach experiment.

Wigner:

There was a paper by Ehrenfest and somebody considering carefully how the angular momentum

could be transferred to it so that it becomes quantized.

Dirac:

I don't remember that paper. There was much discussion about the Stern-Gerlach experiment.

Wigner:

This was not the problem which was immediately in your mind, which was foremost in your mind, uppermost in your mind?

Dirac:

It was one of several.

Kuhn:

But your personal worry was really the helium atom?

Dirac:

I don't want to put too much emphasis on it. I was wondering about all the problems together.

Kuhn:

What was it you tried to forge a better quantum mechanics with?

Dirac:

I don't remember in detail. I think I was trying to develop the Bohr-Sommerfeld method of quantization.

Kuhn:

Do you have notebooks? Do you have things that go back to that period?

Dirac:

I used to work on scraps of paper.

Kuhn:

What happened to the scraps of paper?

Dirac:

I've kept a lot of them. I've got some big piles of them.... Most of it is too scrappy to be able to suggest anything. But there may be some bits which would be useful.

Kuhn:

You speak of problems being widely discussed. I take it Fowler clearly was much involved with them himself.

Dirac:

He was the center.... There were not nearly so many students in those days.... Probably only between three and eight.

Kuhn:

When you speak of there being a lot of discussion of the Stern-Gerlach effect, it was in this group of students and Fowler?

Dirac:

I was thinking of meetings of the Delta2V and the Kapitza Club.

Kuhn:

How big were those meetings? About 20.... They were mainly students, and faculty would come when they wanted to. The Kapitza Club met every week, and the Delta2 v, I think it was twice a term. It was

mainly from these meetings of clubs rather than just discussion groups. The colleges also had their mathematical societies. The undergraduates would get someone to talk. Maybe one of them, or maybe one of the faculty come talk to them.

Kuhn:

Did quantum mechanics become large in these discussions as compared with relativity?

Dirac:

Yes. The Kapitza Club has kept a minute book. I don't know where it is now. You should get hold of that minute book of the Kapitza Club, that would be a list of all the early talks. It's stopped meeting for the last two or three years. But you ought to get hold of that minute book.

Interview Session – 2

Kuhn:

I'd really like to get a clearer idea of what you may actually have covered In school science and mathematics, then later at Birmingham, and at Cambridge.

Dirac:

In the first place it was Bristol, not Birmingham. I went to a very good science school in Bristol, a Merchant Venture's School. They just concentrated on science and on modern languages. They didn't teach Latin or any kind of classics at all. Just a little bit of history and geography. I didn't like the arts side of it. I feel I was very lucky to get to this good school. The school shared the same building with the technical college. It's a school in the daytime and a technical college in the evening so all the laboratories of the technical college were available to the school. We had very good laboratory facilities. Then another thing which helped me was that as soon as I went to this secondary school the war started. That meant that the older boys were taken

away, called up for war work, the higher classes were empty and they pushed on the younger boys as quickly as they could. So I got pushed on ahead and learned science earlier than I would otherwise have done.

Kuhn:

Was it quite unusual to have no classical languages in a school of this sort?

Dirac:

Well, most secondary schools would teach Latin. This one didn't.

Kuhn:

Did. that mean that it was designed to prepare people particularly for technical schools?

Dirac:

Well, it was meant to give a good scientific education. I don't know of any school now which concentrates so much on science as that one did.

Kuhn:

But would it mean, for example, that students who had graduated from there generally could not go to Oxford or Cambridge without Latin in that day?

Dirac:

They would have to take Latin as a separate subject; not everyone had to go into it.

Kuhn:

What sort of schools would most of them have gone to?

Dirac:

... They would go to Bristol University or other modern universities.

Kuhn:

Would. they be particularly likely to go into the sciences or engineering?

Dirac:

They would from that school.

Kuhn:

Can you tell me more about what subjects you would actually have prepared there in the sciences?

Dirac:

No biological sciences. That hadn't come into the ordinary school work then, but (we had) physics, chemistry, mathematics; mathematics was divided into algebra, geometry, trigonometry. Of course I was soon working independently in mathematics — independently of the class. I was just given books to read which I worked through by myself.

Kuhn:

Do you remember what books?

Dirac:

Well, there was Edwards Calculus, differential calculus and Integral calculus. I think it was Hall and (Knight's) Geometry, I'm not sure. I don't remember very clearly what the books were. I know we did not stick to Euclid, We had more modern methods of geometry than Euclid. There were mainly the science courses and courses in English, French and

German. We had three hours a week in each of these subjects.

Kuhn:

How much would have been in the physics course?

Dirac:

There may have been three hours a week of lecturing and one afternoon of practical work.

Kuhn:

Just for the one year?

Dirac:

I think all the time. Chemistry also I started quite early because of being pushed on on account of the war. I remember my chemistry master, Mr. Boyland, believed in teaching chemistry in the modern way and he introduced atoms and chemical equations very early, in the course. I think in almost my first lecture in chemistry he introduced me to these things. We learned about atomic weights and we did not learn about equivalent weights. I think even now in school in chemistry they spend a lot of time on equivalent weights and only go on to atomic weights

later on. I know my daughter was learning quite a lot about equivalent weights.

Kuhn:

Well, I think I had them both but I think I had atomic weights before I had equivalent weights.

Dirac:

Yes. I hardly ever heard of equivalent weights. I hardly knew what they meant. Well, do you have any other questions?

Kuhn:

What sort of subjects in physics would you actually have gotten - through using how much of the accompanying mathematics either by yourself or in the course?

Dirac:

It was only in mathematics that I worked by myself, in the other subjects I worked in the class. I don't remember. I suppose they were just the usual things. Heat, and light and sound. It was spread over four years, you see. I had four years in that school.

Kuhn:

Where you worked by yourself in mathematics in that school, did you then come back and talk to the teacher about this or did you simply tell him that you had read the books?

Dirac:

I expect I talked to him a bit. I think I was mostly on my own. I was just allowed to read what interested me. I took the same exams as the rest of the class. They were no problem to me.

Kuhn:

Were you reading at this time by yourself in other subjects? I don't necessarily mean academic subjects. Did you read novels? Or poetry?

Dirac:

I read some novels. In fact, one was supposed to do it in connection with the English lessons. I was always a slow reader and I didn't like poetry. In fact, I never understood good poetry.

Kuhn:

Well, this was not something you went on with, particularly then on your own as you did with the math.

Dirac:

No, no. My whole interest was on the scientific side. I was really very ignorant of matters outside my schoolwork.

Kuhn:

I think you were also quite interested at times in economics.

Dirac:

Not at that stage.

Kuhn:

When did that begin?

Dirac:

I was never very much interested in economics. Who said that I was?

Kuhn:

I'm not sure. You gave an economic example in your Nobel address, for example. Someone spoke also of an occasion when you had talked on, I think, an economic, perhaps a social, problem when you might at least equally well have talked about matters more strictly scientific.

Dirac:

It must have been much later because... Certainly not in that period at all. I took no interest in political things either.

Kuhn:

Now let me ask you about subjects and courses at Bristol.

Dirac:

In the school or university?

Kuhn:

Now I mean at the university, assuming that we have done this now for the pre-university.

Dirac:

Well, I was in electrical engineering at Bristol and that was in the same building again as this school and the technical college. They were all together. The faculty in engineering was quite small then. They were all housed in the same building so that when I went to the university I continued to go to the same building. To some extent I had the same teachers. I specialized in electrical engineering and I was mainly influenced by the professor of electrical engineering there called David Robertson. He was extremely methodical. He taught both theoretical and practical work. I remember how methodical he was with the practical work, impressing upon people the need to arrange things so as to avoid the possibility of accidents. He was paralyzed from polio and he rode around everywhere. He was paralyzed in his legs and he went around everywhere in a wheelchair. So he had to arrange his own life methodically.

Kuhn:

This made no great problems for him in teaching the practical work?

Dirac:

Well, he could go around in his wheelchair and see what everyone was doing and explain things to them.

Kuhn:

How theoretical was the teaching for electrical engineering of electromagnetic theory or how much of electromagnetic theory as such did you get in that curriculum?

Dirac:

Well, engineers just concentrate on the equations that give the right answers. They don't want to know the reasons for those equations. I learned the equations and how to use them — all about inductance, capacitance and so on, how to work out electrical circuits and the rules for winding electrical motors and dynamos. I found it was quite interesting mathematics in some of these rules. I am grateful to David Robertson for explaining these things in a way which did show the mathematical beauty of some of the things which one had to calculate.

Kuhn:

Did one go as deep as Maxwell's equation?

Dirac:

We didn't do electrical waves at all. Wireless didn't exist as a regular thing.

Kuhn:

So this was really for power engineering.

Dirac:

Yes, yes.

Kuhn:

Did you learn to do what I once learned to call Heaviside calculus?

Dirac:

I did a bit of that with solving differential equations, linear differential equations with constant coefficients. I was just taught it as a rule that worked. I couldn't explain very well why they did work, but still they did work and gave the answer

that you wanted, and that was enough to satisfy engineers.

Kuhn:

Were you impressed with that?

Dirac:

There is some sort of magic about it, yes. It was strange how you could get the answers out.

Kuhn:

I had just a taste of it and didn't really get back to it until I began to learn a little bit about operator techniques and delta functions. You had these in the reverse order and I wondered whether they had any kinship.

Dirac:

Well, I was just first taught the way to — the rules which one had to use to get the right result.

Kuhn:

I've got no notion how specialized or how quickly specialized the electrical engineering curriculum

would have been. Did you do simply electricity or did you...

Dirac:

Well, there was some amount of general engineering, for instance, testing materials, pulling things until they break and calculating the breaking stress, and things like that. We had quite a good deal about calculating stresses in structures.

Kuhn:

Would you have had a general dynamics course along with this?

Dirac:

Yes, yes. One had to know about the dynamics. There was quite a good deal of general engineering in the work.

Kuhn:

Was the approach to the scientific problems generally rather different from what one would have had if one had been in physics or was this quite a lot the same?

Dirac:

I think it was different in the sense that they just concentrated on learning equations that give the right answer. Then you could proceed to work out the answer with slide rules.

Kuhn:

Were you at all impelled yourself at that point to push back to a more basic source for these formulas?

Dirac:

I don't think I felt impelled to it. I think at that time I wasn't really doing much inquiring; it was just absorbing the knowledge which was given to me. I think it was! probably that sort of training that first gave me the idea of a delta function because when you think of loads in engineering structures, sometimes you have a distributed load and sometimes you have a concentrated load at the point. Well, it is essentially the same whether you have a concentrated load or a distributed one but you use somewhat different equations in the two cases. Essentially it's only to unify these two things which sort of led to the delta function.

Kuhn:

Do you think you did try to unify them at that time?

Dirac:

Not at that time. Oh, no. Not at that time at all. But I just felt that they were essentially the same even though the treatment was different. There was a lot to be done just calculating stresses and things like that. Maybe the stress on a whirling shaft, and I found that some of the mathematics was quite pretty even though the whole work was approximate. I think I owe a lot to my engineering training because it did teach me to tolerate approximations. Previously to that I thought any kind of an approximation was really intolerable. One should just concentrate on exact equations all the time. Then I got the idea that in the actual world all our equations are only approximate. We must just tend to greater and greater accuracy. In spite of the equation's being approximate they can be beautiful.

Kuhn:

When you say approximate here, I think people often use the same term in two rather different ways.

This may mean that they give approximate results in the sense that engineering equations do. It may mean that the whole physical theory with which the equations are involved is approximate in the sense that it's an approach to what's really there.

Dirac:

Well, it's just neglecting a lot of factors. I meant it in that sense. The actual situation is far too complicated, you have to neglect a lot of factors.

Kuhn:

Would you say that one gets the Bohr atom from quantum mechanics or that one gets to quantum mechanics from the Bohr atom by adding simply a factor that one has been ignoring previously, which would give a sense of approximation?

Dirac:

You mean by quantum mechanics the Schrödinger equation.

Kuhn:

The Schrödinger equation or the general -. I mean would that also be an example of an approximation in the sense that you have in mind?

Dirac:

Yes, it would be. Yes.

Kuhn:

Because clearly the logical relation of the earlier and later theory there is more complex; that is, it would be difficult to isolate a single — or group of extra things, that sort of added on

Dirac:

I think it's very likely that all our equations are only approximate. Our present quantum theory is probably only an approximation to the improvement of the future. I feel that everything might be an approximation and this comes very largely from the engineering training.

Kuhn:

I think this question may arise again when we come back to some of the problems presented by quantum mechanics. You may be willing to elaborate it because I think most engineers' idea of what is involved in the approximation is that there may be a complete theory, an exact theory, which you must know in order to know what you're neglecting.

Dirac:

I don't think that's the engineering approach. I think they have a general feeling about what is important and what can be neglected. That would make a good engineer. Probably would make a good physicist also.

Kuhn:

But this is a sense of an approximation in which one doesn't ask except perhaps intuitively to know what the full series would be in order to justify dropping the term.

Dirac:

Yes, yes. You just rely on your general intuition. That's certainly so for engineers.

Kuhn:

Very good. In addition, were you doing anything over and above your engineering studies at this point as you had gone ahead by yourself with math at school?

Dirac:

I continued to go ahead with math and I was interested in relativity. You see, just at that time relativity sort of burst on the world. That was 1918 when the war ended and relativity had a tremendous effect on the general public. News- papers were full of articles on relativity and magazines also. Every one was talking about relativity, It was a new philosophy and it produced more excitement I think than science has ever produced before or since. Of course, engineering students were also interested in it and talked about it. And during in my later period I worked on it. I told you that I went to Professor Broad's lectures. remember I was also interested in

other things, philosophy and logic. I got a copy of Mill's from the library and read that all through. I found it pretty dull, but still I stuck to it, and tried to understand these things. I thought at that time that maybe philosophy was important. Since then the field of philosophy has terribly declined. I feel that philosophy will never lead to important discoveries. It's just a way of talking about discoveries which have already been made.

Kuhn:

What else did you read in philosophy?

Dirac:

I can't remember reading any other books.

Kuhn:

Were there philosophical problems that you talked to people about or worried about yourself? Bohr, for instance, talked a good deal about how concerned he had been at one time with the problem of the freedom of the will.

Dirac:

I never talked to people about it. I thought about it, but found that sort of problem insoluble. I did think a lot about these things but I never came to any definite conclusions.

Kuhn:

Do you remember what the particular problems were?

Dirac:

I think I told you that one time I thought that space and time might be connected and that you might have to rotate space and time axes together. But I only knew about Euclidean space then and, of course it won't work with Euclidean space. So I had to give up with that.

Kuhn:

Were some of the more usual philosophical problems like the freedom of the will, the reality of other minds?

Dirac:

I thought about those things, yes. Can't remember anything definite that this thinking led to.

Kuhn:

You spoke, when you talked before about relativity, about having gotten hold of Eddington's book.

Were there other books at that point that you were particularly working in yourself?

Dirac:

I don't think so, no. Eddington's book was the first mathematical book on relativity.

Kuhn:

Well, I've noticed when you get up to Cambridge you start publishing quite soon. Your second semester. Now clearly you've got a very good command of statistical mechanics. Did that all happen while you were there?

Dirac:

No. I might say that there was a good deal of controversy at that time between Eddington and

Jeans and some of the students were very interested in that. One of the students that I was especially friendly with called Wilshire used to be very keen to read letters in Nature which Eddington and Jeans had written.

Kuhn:

This was a controversy over relativity?

Dirac:

I've forgotten what it was about. No, I think it was some astronomical thing. One can look it up. Wilshire was also interested in relativity and we had talks about that together.

Kuhn:

You said that you thought your choice of engineering was probably largely determined by the fact that your brother had done this.

Dirac:

Yes, and also it was in the same building and a natural thing for students to go on to. I suppose I didn't know then that one could get one's living in pure science.

Kuhn:

Had you been tempted to do mathematics, for example? It was the thing you had been reading by yourself.

Dirac:

It was the thing which interested me most but I did not know that one could get a living by it. No one ever spoke to me about it. Engineering was something which one could earn one's living from 'and I knew that.

Kuhn:

Well, teaching mathematics, would you have thought of that as a way of making one's living?

Dirac:

I didn't like teaching very much. Of course, I thought I could have been a school teacher but I didn't like it.

Kuhn:

So you think probably that you considered mathematics as a possibility and rejected it because

it meant school-teaching which you didn't want to do.

Dirac:

Yes.

Kuhn:

How did you feel about the practical work in engineering? You've spoken of the appeal of engineering approximations, but what about the manipulative, mechanical "soldering-side" of electrical engineering?

Dirac:

I don't suppose I was very good at it. On one long occasion I went to a factory to get some practical training in engineering work. They sent an unfavorable report on me to my professor, so I presume I didn't do very well.

Kuhn:

Did you enjoy the more manipulative part —.

Dirac:

I enjoyed it, yes, but I was extremely ignorant of anything which I hadn't learned in class. I was just lacking in., common sense, I suppose.

Kuhn:

Did you ever do experimental work after you made the transition to physics?

Dirac:

I did a very little at one time, yes.

Kuhn:

I ran onto a letter, which I think was printed in the Fermi volume, from Rutherford to Fermi in response to some news about the early neutron work.

Rutherford congratulates Fermi on turning to experimental work and then says that you're also. doing some experimental work so there's hope for theoretical physics.

Dirac:

Yes. I did a little work on trying to separate isotopes by centrifugal methods.

Kuhn:

How did you core out with that? I know we're out of chronological order, but we're not likely to come back to this one by the logic of your paper.

Dirac:

Well, I was a close friend of Kapitza at that time and I think he interested me in it and persuaded me to take up some experimental work; I had the idea that maybe just the rotation of gases at high speed without any mechanical moving parts could be used for separation of isotopes. I did some experiments on it and got a negligible amount of separation, but I got another effect which I wasn't expecting which was interesting, namely a thermal effect. I was able to produce something like a conjuring trick. I just showed three pipes, pumped in some air into one pipe; have the one pipe here branching out into two pipes and the air would come out of these two pipes at different temperatures, widely different temperatures.

Kuhn:

They were coming off symmetrically?

Dirac:

Well, I mean there were some asymmetry in the apparatus but the apparatus was so small that it was concealed- . I believe we had differences of about maybe 1000 centigrade.

Kuhn:

At what temperature?

Dirac:

The pressure was about six atmospheres. Maybe not quite that much; but below freezing point on one side, and the other side quite high. Comes entirely from the viscosity effects. The inner layers have a higher angular velocity than the outer ones and the viscosity transfers energy from the inner layers to the outer ones. The outer ones go out one side and the inner ones come out the other side and there's quite a big temperature difference.

Kuhn:

Did anything further come of that?

Dirac:

I tried to see whether there was any separation when one used a mixture of gases and it seemed to me the simplest way to try that would be to put some scent in it, the scent being some heavy gas, and see whether more scent comes out one side than the other but it turned out later that it was just due to the fact that one's sense of smell is very much keener at low temperatures than at high temperatures. The temperature effect quite dominated any other effect.

Kuhn:

When you say you had thought of mathematics and felt that there was not a living to be made there, was physics a career one would even think of at this time?

Dirac:

I never thought of it. No one ever spoke to me of it as a career. I did not come into contact with the physics and mathematics people at the university because they were in quite a different building a half a mile away. I never went to that building. I was entirely at this Merchant Venture's College.

Kuhn:

Do you mean to be taken literally when you suggest that its being in the same building was perhaps a major factor in the choice?

Dirac:

I think it was quite an important factor, yes. And the same people, same staff, to some extent. This David Robertson, for instance, who was my physics teacher in the school, I also —.

Kuhn:

So it was really because these were the subjects you enjoyed and you went right on with them.

Dirac:

Yes. With the same professor.

Kuhn:

I see that more clearly now. I'd taken it initially to mean, 'Why go a half mile away to another building?' But I see much more now the sense of continuity in a subject which you liked and were doing well with.

Dirac:

Yes. The people there rather took it for granted that I would continue in that way.

Kuhn:

Well, the last time you talked, you said that you had heard later that the mathematicians were quite disappointed that you decided to do engineering,

Dirac:

Yes, yes.

Kuhn:

This sounds as though the mathematics was once a more live option, as though they might have either persuaded you to do it or you had talked about doing it or something.

Dirac:

They had heard about my good examination results and they hoped that I would specialize in mathematics.

Kuhn:

But they didn't try to persuade you?

Dirac:

I never heard anything of it. Perhaps it's going too far to say they never did anything, but if they did, it's skipped my mind. Well, going back to this centrifugal force, there's one further thing to add. When Kapitza was detained in Russia, I rather stopped. I didn't have enough enthusiasm to carry on without him and so nothing further was done on it until the war came and people wanted to separate isotopes of uranium and the subject was taken up again some modified apparatus was experimented with in Oxford. I went to Oxford from time to time to talk with the people there who were doing experiments. There is a possibility of getting small effects from the separation of gases by rotations which are produced without any mechanical moving parts.

Kuhn:

It never, I take it, was done on a major scale?

Dirac:

No, I don't think it was good enough to compete with the diffusion method. That was all the experimental work that I did as a physicist. Of course, I had to do a lot during my training in school and in the university.

Kuhn:

You said before that when you first went into the school of mathematics at Bristol, you really started first by going back to engineering school because you had not been able to get an engineering job in the depression.

Dirac:

That is so, yes. This professor, David Robertson, told me I shouldn't waste my time hanging around doing nothing and that I should go back and do some research. —He started me off on a problem with Stroboscopes, and after a few weeks there the mathematics people asked me to go and study mathematics with free tuition.

Kuhn:

Well, this is clearly the beginning of your way back or your way to physics. But I take it earlier if you'd suddenly heard of an engineering job, you would have taken it.

Dirac:

Yes.

Kuhn:

Is there anything identifiable as a point of decision?

Dirac:

Well, I really went over to mathematics in that period, not physics.

Kuhn:

Yes. But is there a point at which it suddenly becomes clear to you that you're not going to go to an engineering job if it does become available?

Dirac:

No, I don't think there's any definite occasion like that. I stopped applying for jobs when I went to study the mathematics at Bristol.

Kuhn:

And so far as you remember you never did open that up again.

Dirac:

No, I never applied again. Well, having started on a course, I wanted to finish it.

Kuhn:

That was a two-year course from the beginning, was it?

Dirac:

Yes. It would normally be a three-year course but with my previous training they let me off a year. There was another choice that had to be made, because in the first year of that two-year course one did pure and applied mathematics together, half and half. The second year one had to specialize in either

pure or applied. So I didn't really know which to choose. I didn't mind very much between then. There were just two students doing the honors course then, a girl, called Miss Dent, and me. The professors, of course, wanted both of us to choose the sane option because it would mean only half as much teaching for them to do. But Miss Dent was very definite that she wanted to do applied and that was why I did applied mathematics rather than pure. Again, it's rather fortuitous that I got onto the path of applied mathematics.

Kuhn:

What subjects did you do — this would be a course that met every day?

Dirac:

Oh, yes. There were lectures every day, I think. Well, when I was doing both pure and applied, I was very much influenced by Peter Fraser whom I told you about. I learned projective geometry from him and I found it very interesting. It was really a fascinating subject. I found an article about Fraser (looks for article). You might like to look at it. You can take it along and read it.

Kuhn:

Thank you. Do you remember where it's from?

Dirac:

Hodge wrote it, and you could ask him. Professor Hodge is in Cambridge.

Kuhn:

Well, I shall take that along and have a look at it and bring it back. I'm very glad to see it.

Dirac:

I'd say he was the best teacher I ever had.

Kuhn:

Did you have him just in this one course?

Dirac:

No, he taught other things also, rigorous foundations in mathematics, how to differentiate and integrate with rigor. I previously had just learned the rules well enough for engineering and I found it rather hard to appreciate that rigor was needed. It seemed to me that when you were confident that a certain

method gave the right answer, you didn't have to bother about rigor. In fact, I still feel rather that way. But Fraser did a: very good job of persuading one of the need of rigor.

Kuhn:

Did any of your math get you more toward algebra?

Dirac:

By algebra do you mean sort of noncommutative stuff?

Kuhn:

Well, at least something more - just a class of variables that don't necessarily have to be numbers.

Dirac:

I remember reading about quaternion's by myself. I never had it in class but I did get hold of an old book on quaternion's.

Kuhn:

Do you have any notion what that would have been

Dirac:

Well, there weren't very many books on quaternion's. Who was it, Thomson, Tait?

Kuhn:

There are some quaternion's in Thomson and Tait.

Dirac:

This is a book entirely on quaternion's. A big thick book,

Kuhn:

That thick book on quaternion's would not be Thomson and Tait.

Dirac:

There weren't very many books on quaternion's in those days. They talked about tensors and versors. It was an old-fashioned book.

Kuhn:

Good. I'm much interested. You've mentioned Thomson and Tait and knowing something about quaternion's from that last time we talked —.

Dirac:

Maybe Thomson and Tait is the wrong book and I read something else.

Kuhn:

But you do remember there being a book on quaternion's by themselves. How did you like that?

Dirac:

I liked it in some ways, but I didn't appreciate it to the full extent because the authors did talk about tensors and versors for the scalar and vector parts and they rather separated the scalar and vector parts too much, instead of thinking of the two together as one entity. They put the wrong emphasis on it. So I didn't appreciate it as much as I ought to have done if I'd had a better book.

Kuhn:

This connects with something you said last time that puzzled me a good deal, because I think in talking about the projective geometry with Fraser, you said that ever since then your own approach has been largely geometrical.

Dirac:

Yes.

Kuhn:

I was somewhat puzzled about this because I would myself have thought of your approach as being very often algebraic, but I probably wouldn't have even raised this except that as you may know Oppenheimer arrived in Copenhagen just before I left. He sends you his greetings. We were talking bit together about you and he came out, without my having raised this at all, with a remark about your immense facility as an algebraist. This has been somewhat my own feeling but runs dead counter to this remark of yours. I wonder if I could vex you by asking you to say a little bit more about what you'd had in mind.

Dirac:

Well, I'm not altogether sure what is meant by 'algebraist'. If it means some- one who simply carries through masses of algebraic calculation without picturing what the equations mean, then I'm just no good at it. All this modern work about

dispersion relations and reggi-poles and things like that I find very difficult to follow. It doesn't impress me strongly at all because I don't see the geometrical connections.

Kuhn:

I would think of your peculiar q -number manipulations, for example, as being algebraic rather than geometric.

Dirac:

Yes, but I only used them in an elementary way. Perhaps I didn't tell you that I kept up my connection with geometry some time after I came to Cambridge. There was a Professor Baker, a professor of geometry, who used to give tea parties on Saturday afternoons to people who were keen on geometry; after the tea someone would give a talk on some recent research work on none geometrical subject. I went to those tea-parties and absorbed quite a lot of geometry then. I talked once or twice myself. I remember I worked out a new method in projective geometry and gave a talk about that at one of these meetings. I never published this method. Well, that's a good deal about working with the

geometry of four or more dimensions. Four dimensions were very popular then for the geometrists to work with. It was all done with the notions of projective geometry rather than metrical geometry. So I became very familiar with that kind of mathematics in that way. I've found it useful since then in understanding the relations which you have in Minkowski a space. You can picture all the directions in Minkowski space as the points in a three-dimensional projective space. The relationships between vectors, null-vectors and so on — and you get at once just the relationships between points in a three-dimensional vector space. I always used these geometrical ideas for getting clear notions about relationships in relativity although I didn't refer to them in my published works.

Kuhn:

Did this also give you techniques that were relevant to your approach to quantum theory whether relativistic or not?

Dirac:

No. It doesn't connect at all with non-commutative algebra.

Kuhn:

It is all right to think of that as being algebraic.

Dirac:

Yes, yes. But I don't think you'll find any heavy algebra in any of my work.

Kuhn:

No. There is an awful lot of algebraic sense, though it's hard to know how to put that more precisely; but I think with many people who find the approach to a multiplication relation more general than XY , where these cannot be thought of as numbers, it suddenly gets away from them as a subject matter. You've clearly made this exception with ease and facility, at a time, I take it, when it wasn't the standard thing to do.

Dirac:

Yes, I suppose. Non-commutative algebra was a rather strange idea in those days, although it shouldn't have been because of quaternions. Hello, come on in. (Someone enters room)

Kuhn:

I think we were talking about the transition from engineering to math and I think it might be appropriate to go on now to the transition from math to physics.

Dirac:

When 'I came to Cambridge, yes.

Kuhn:

I take it that although you had had a year in which you turned out to be in applied math, this wasn't necessarily to be a career with the subject matter that you ultimately went on with.

Dirac:

Not necessarily, no.

Kuhn:

Did you think of yourself as going to Cambridge to do applied, math?

Dirac:

It was already when I was finishing my engineering course, my father sent me to Cambridge to try for a scholarship. Actually, it was too late when he thought of it to try for the honorary scholarships because the examinations for the honorary scholarships are in December but St. John's College offered some exhibitions for which the examinations were held in June. I went and took that examination and they offered me an exhibition of 70 pounds a year. I wasn't able to get any other money to supplement it and it wasn't enough to come to Cambridge with. That's why I stayed on in Bristol. But after the two years in Bristol I was awarded a government grant from the Department of Scientific and Industrial Research and with the help of that I was able to come to Cambridge.

Kuhn:

Could you also take up the previous scholarship?

Dirac:

Yes. The previous exhibition. They allowed me to take it up two years later. It was still a very small amount of money, but just enough to manage with.

Kuhn:

Was your father able to help you at all?

Dirac:

He didn't seem to want to anyway and I did get by entirely, in this way, independently of my father. He left quite a bit when he died, but at that time he didn't think he could afford it.

Kuhn:

If you had gone to Cambridge immediately after finishing your engineering course, would you have gone into applied mathematics or mathematics?

Dirac:

To begin with, one doesn't specialize. I would have had to specialize later. don't know how it would have turned out.

Kuhn:

Well, since you had already finished your engineering work, you'd have had some idea of going to study something.

Dirac:

It would have been mathematics, yes.

Kuhn:

Well, in that sense there really wasn't as much accident involved as I had thought before in the transition from engineering to mathematics in the graduate school at Bristol.

Dirac:

That was made quite independently of my having won this exhibition in Cambridge.

Kuhn:

But at least you would have gone on to do mathematics at Cambridge. Well, coming back now to when you actually do go there in '23, you'd had a good deal of mathematics in the meantime. Was it the sort of mathematics by that time that meant that you were thinking of a career in teaching?

Dirac:

I didn't like the idea of being a school-teacher anyway.

Kuhn:

Did that seem the likely outcome?

Dirac:

Well, if I wasn't very good, it might have been the only possibility. I didn't like the idea of becoming a schoolteacher.

Kuhn:

The other alternative was university teaching?

Dirac:

Yes. I didn't know whether I'd be good enough for that.

Kuhn:

Clearly you had impressed an awful lot of people quite a lot; was there real doubt in your own mind about the likely outcome of this?

Dirac:

I was really very ignorant of the world. I had done well on examination but that was all. I had no idea what the standard was in Cambridge.

Kuhn:

You went there more or less with an eye to continuing in more advanced subjects in mathematics?

Dirac:

To do research. The government grant that I got was specifically for doing research

Kuhn:

You didn't have to have a particular research project.

Dirac:

No, no.

Kuhn:

It was research in mathematics, but it might have been anywhere in mathematics.

Dirac:

Yes, yes. So I came to Cambridge as a research student and not as an undergraduate. That was why I was able to come without knowing Latin. If I had gone to Cambridge earlier as an undergraduate I would have had to suddenly learn Latin.

Kuhn:

Did you attempt to pick up research problems virtually on arrival, or did you go to lectures and read at first?

Dirac:

Well, Foulser was appointed as my supervisor.

Kuhn:

This was immediately on arrival.

Dirac:

Yes. I think perhaps I first went to see Cunningham. I had seen Cunningham previously when I came for the examination for the exhibition. I was rather hoping that Cunningham would be my supervisor. He worked in electromagnetic theory and I thought

that was an interesting subject. Cunningham couldn't take me. I don't quite know why. I was assigned to Fowler and his interest was statistical mechanics and I thought that to be a relatively ugly subject.

Kuhn:

Had you looked at it at all before?

Dirac:

Not really, no. Cunningham is still alive, by the way; if you'd like to ask him any questions you probably could. He lives here in Cambridge. Well, Fowler set me to think about this question of dissociation under a temperature gradient.

Kuhn:

At the very start?

Dirac:

Pretty near the beginning, yes. He also gave me things to read and lectures to go to. Fowler himself was giving lectures on quantum theory, I believe. It was then that I first learned about the Bohr atom. I

was quite surprised to find that atomic theory had developed to such a large extent.

Kuhn:

Do you still have notes on those lectures?

Dirac:

It's possible that I have them. I don't remember where they are; I could look for them.

Kuhn:

It would be terribly helpful to have a precise idea. at what level, over what illustrations and subject matter Fowler was actually lecturing at that quite key point. So if you do have those I think we would like very much to microfilm them.

Dirac:

I also went to Cunningham's lectures. He was lecturing on classical electrodynamics. I went to lectures on thermodynamics. I believe that was by Newman. Newman gave some lectures on relativity and so on. He is now in Manchester. I went to the Colloquia at the Cavendish. We sometimes had visitors from foreign places. Bohr came and lectured

sometimes. Of course, I was very happy to meet Bohr. I was also very happy to meet Eddington, because I had heard about him. so much in my engineering student days. Eddington was the great man then. He 'brought relativity to England, and he led that eclipse expedition to check the Einstein effect in 1919. That aroused tremendous public interest.

Kuhn:

Did you go to lectures of Eddington at that time, or courses of lectures?

Dirac:

I can't remember whether I did or not. I don't remember now.

Kuhn:

Well, there are all sorts of subjects coming in quickly now that are really new to you. I mean the electromagnetic theory ties somewhat to your previous engineering training, but must go a great deal beyond it. Statistical mechanics is a subject which was perhaps quite new.

Dirac:

Yes, yes. Lennard-Jones was in Cambridge at the time. I learned quite a bit from him. Well, I got introduced to the Boltzmann equation for the first time. 891 This was a starting point for statistical mechanics. I also learned some astrophysics from Milne. During one term when Fowler went to Copenhagen Milne was appointed my supervisor and he set me on that astrophysical problem on which I wrote a little paper.

Kuhn:

Unless my impression is wrong, when you came to Cambridge you start on more advanced work, but really the whole subject matter broadens out.

Dirac:

Yes.

Kuhn:

Now, at least from the point of view of the American vocabulary, you are doing physics as you had not previously been doing physics. Many of these subjects are applied mathematics or astronomy at

Cambridge — this is not the natural way to describe it.

Dirac:

Well, the electromagnetic theory was really physics.

Kuhn:

Could you have had many of these same subjects at Bristol or did they simply not exist there?

Dirac:

Statistical mechanics and thermodynamics I don't think existed at Bristol.

Kuhn:

Electromagnetic theory probably did?

Dirac:

Yes, but not the waves. Not electromagnetic waves. I learned a little about the lagrangian and a little about the Hamiltonian.

Kuhn:

But that existed in the sense of potential theory problems.

Dirac:

Yes, quite a lot of potential theory problems.

Kuhn:

Well, I would be grateful if you could look at some point and see what lectures you may have notes on. It would help to pin down content of lectures more specifically and there may very well be a few of those that we'd like to microfilm.

Dirac:

I think my notes are pretty rough. In any case, I didn't absorb very much from lectures. I got a more thorough understanding from private reading than from lectures.

Kuhn:

Do you remember again books you read in this period?

Dirac:

Sommerfeld was the main book. Sommerfeld's Atombau and Spectralinien.

Kuhn:

You read that in the German. There was an English translation-

Dirac:

It was an English translation, but it was rather a bad one. I think I read it in the German. I had read some German in school and it turned out to be rather inadequate. Still, with the help of a dictionary, I was able to plod along and gradually got more fluent with it.

Kuhn:

But Fowler's statistical mechanics was not out yet.

Dirac:

Oh, no. Not till much later. But I read a good many papers and periodicals.

Kuhn:

What periodicals would you have been likely to follow at that point?

Dirac:

The Proceedings of the Royal Society, and the Zeitschrift fur Physik, that was the main physics journal in those days. Annalen der Physik I recall. I don't know whether the Physical Review existed then. It wasn't very important.

Kuhn:

Yes, it did exist, but I find almost nobody who would have been likely to be reading it in Europe. It had existed for a while, but was not an international journal.

Dirac:

Well, the Zeitschrift fur Physik was the main physics journal in those days. And it was from reading these papers that I got ideas for problems in research work.

Kuhn:

Some of those also must have come from teachers.
That is, Fowler you say put you on right at the start.

Dirac:

He definitely put me on this first problem as soon as I arrived.

Kuhn:

Is it that second one that is the relativity dynamics of a particle?

Dirac:

That's just a very short note. That was suggested by something that Eddington had written and Fowler had talked about. Seems that I could put things a little more clearly than he had them. Yes, it was in Eddington's book.

Kuhn:

That came out of the book you think rather than from taking a course with him?

Dirac:

I think it came out of a book, yes.

Kuhn:

The paper you do on the Doppler principle and the Bohr condition that may well come out of reading.

Dirac:

Yes, what was the title of that paper?

Kuhn:

“Note on the Doppler Principle and the Bohr Frequency Condition.”

Dirac:

I expect it did. I’ve forgotten that paper. Was that in the Cambridge

Kuhn:

Yes. That’s the

Dirac:

I think that did come from private reading.

Kuhn:

Well certainly there is a relevant paper of Bohr's that you point to that's In the supplement to the Cambridge Proceedings.

Dirac:

Yes, I think this sort of work came just from reading things and noting that it's possible to improve what someone else has written

Kuhn:

In connection with your next paper on the detailed balancing in the Proceedings of the Philosophical Society [Royal Society A 106], I'd like to ask whether by any chance you knew of de Broglie's work in this period.

Dirac:

1924, was it?

Kuhn:

Yes.

Dirac:

I did know about it and I wasn't strongly impressed by it because it seemed to me to be a mathematical curiosity. I didn't take these waves as something physically real in the way that Schrodinger did. It never occurred to me to try to generalize it for particles which are not moving uniformly. I looked upon it just as a mathematical curiosity.

Kuhn:

Do you remember how you knew of it, because there are lots of people who were totally unaware that it existed in this period.

Dirac:

I think de Broglie had published something about it. I had read about it.

Kuhn:

Do you remember where?

Dirac:

I don't remember where, no.

Kuhn:

The thesis itself, although it was published in full in French, was not often read; there was however an article in the Phil. Mag., that summarized it.

Dirac:

I probably got it from that.

Kuhn:

Fowler was the one who transmitted the article to the Phil. Mag., I think it was Ellis that was responsible for its being there in the first place because he had been at Maurice de Broglie's lab. But I wonder whether you had ever heard Fowler speak of it or remembered other conversations about it.

Dirac:

No, I don't remember Fowler speaking about it. I don't think it's the sort of thing that would impress Fowler Fowler's interests were in statistical mechanics and the Bohr orbits also.

Kuhn:

Well, of course this tied to both of those, you know. That is, de Broglie had a derivation for the quantum condition or for the Bohr orbit in terms of the interference and he also got some quite interesting statistical results. So I'm not sure whether those were in the article or not.

Dirac:

I didn't know he had any statistical results. I think it was just speculative what he said about the possibility of connecting these waves with quantum condition. It was just a speculation. I don't think people took it seriously. Nobody except Schrodinger took it seriously.

Kuhn:

Einstein took it fairly seriously, I think. There are an awful lot of people who simply didn't know it existed. There was nothing equivalent to the Phil article printed in any German journal that I know of. So by and large it was unknown to people who didn't follow the French literature. I was really led to ask you this question in the first place because of

the one passage in the paper on detailed balancing in which I supposed probably you were speaking directly to the de Broglie paper.

Dirac:

I don't think I referred to it in any [part of the paper].

Kuhn:

You certainly didn't cite it. It was this on page It's that paragraph. [End of § 5. "For the discussion of equilibrium problems, quanta of radiation cannot be regarded as very small particles of matter moving with very nearly the speed of light."]

Dirac:

Well, so far as concerns the light quanta and the electromagnetic waves, de Broglie's ideas were common to everybody but the idea of extending them to other particles with non-zero rest mass was a new thing, which people, did not take seriously.

Kuhn:

But de Broglie, so far as I know, is the only one who in talking about light quanta talks of them as having

a very small but finite rest mass and a velocity correspondingly just a little bit less than the velocity of light. That is, most people simply give them the velocity of light and zero mass. De Broglie from the start talks of having a very finite mass and a velocity slightly less than the velocity of light and you phrase this point on what I agree is in general a well-known idea of light quanta. But this phraseology strongly suggests to me that maybe you're thinking of de Broglie's version of it.

Dirac:

I'd better think this over and see if I can remember any more about it.

Kuhn:

O.K.

Dirac:

We ought to go and have our tea now.

Interview Session – 3

Kuhn:

There is one thing which came up yesterday in which I understood you somewhat differently from our first talk in Princeton. That was this issue of quaternion's which may or may not be important, but which I'd like to pin down. When we talked in Princeton I understood you to say that you had learned something about them from reading Thomson and Tait while you were at Bristol.

Dirac:

I read some book; I might very well have the wrong title, I don't know. It was a big thick book.

Kuhn:

And it was devoted entirely to quaternion's?

Dirac:

Entirely, yes.

Kuhn:

And you think you will have read that book quite early? The reason I ask that is that John Wheeler once sat down to dinner at M.I.T. next to Egon Orowan who mentioned quaternion's and [who] said that he had once given you Tait's book on quaternion's; It's a book I don't know, but it's a likely book. That of course would be very much later.

Dirac:

It was probably when I was an engineering student that I read this about quaternion's.

Kuhn:

You think you had a whole book?

Dirac:

I didn't read the whole book through. I Just read parts here and there.

Kuhn:

If it was a whole book on quaternion's, it would have to be something other than Thomson and Tait, I think. It could well have been Tait's Quaternion's.

Dirac:

That might be it, yes.

Kuhn:

Do you remember a did you use them at all in working out problems?

Dirac:

No, I did not use them.

Kuhn:

It was simply a question of knowing that they existed and being somewhat interested.

Dirac:

I found them interesting

Kuhn:

But you don't think you'd have used them in working out problems or subject matter for your own benefit?

Dirac:

No. I felt they ought to have an important application; I still feel that way, that they ought to have a more important application than they do at present.

Kuhn:

Do you think that given the relatively greater knowledge now and facility in utilizing tensors, general operator techniques, and so on, that the quaternion as something distinct from this would still have that sort of function?

Dirac:

Yes, yes I do. It is the most general algebra with division, and with associative multiplication. That's what we need in quantum theory.

Kuhn:

Did you ever try working out quantum theory in a quaternion formulation?

Dirac:

I have, but I didn't get any new results from it. Other people have also been interested in that question.

Kuhn:

Does that go back very early, or is that fairly recent?

Dirac:

Well, off and on, a good many times. Quaternion's are something which I continually come back to.

Kuhn:

Did you try it at the time of the very early papers? And when you were doing a q-number algebra, for example, did you experiment with quaternion's?

Dirac:

I expect I did a few years later.

Kuhn:

Actually that again is out of order, but it raises a question which I had wanted to ask you. At the time of your thesis, both in the thesis itself, and about the same time In one paper on q-number algebra that's published in the Cambridge Philosophical Proceedings, you do work on the foundations of a q-number algebra.

Dirac:

Yes.

Kuhn:

Did you go on with that? You don't carry it very far at that me, and I see nothing later that's an attempt, if you will, to behave as a mathematician with respect to this sort of algebra.

Dirac:

At that time I didn't properly realize that this was really exactly equivalent to matrix algebra, and later on when I realized that I thought it wasn't really a subject to study In itself... Mathematicians already

know about matrix algebra, and this was really equivalent to that.

Kuhn:

This is taking matrix algebra in its most general form, to include continuous indices?

Dirac:

Yes, yes; operator algebra algebra of linear operators. You don't have to use continuous indices because you're working in Hubert space, and you only use continuous indices when it is useful because of the physical meaning of the indices.

Kuhn:

When you say the mathematicians already knew all about these, in a sense I suppose they did, but there were a number of manipulative procedures and all sorts of mathematician's questions that might have been asked about existence, convergence and the whole question of what mathematicians know about the Delta function as a legitimate mathematical tool. In this sense, then, there was a good deal of mathematics used in the physics of these problems as it developed which the mathematicians were

really not prepared to stand behind, so that work could have been done there.

Dirac:

The mathematicians wanted a higher standard of rigor than was necessary for the physicists. Well, after a few years Wintner wrote his book which gives, I suppose, practically all one needs to know so far as concerns the bounded matrices.

Kuhn:

That's a book I don't know.

Dirac:

I've got it here. [Goes to find the book.] It's not really adequate for quantum theory, because it deals only with bounded matrices, but it's really a foundation of the subject. One ought to know all about the bounded matrices before one goes onto the unbounded ones. And that's all treated with complete rigor. I've often referred to that book when I wanted to know just what is certainly true with absolute rigor.

Kuhn:

Good. For the record I'm just going to read the title: it's A. Wintner, Spektraltheorie der Unendlichen Matrizen, Leipzig, 1929. No, I don't know this book at all.

Dirac:

The 'Unendlich' means an infinite number of rows and columns, but it's still a bounded matrix.

Kuhn:

Did you ever involve yourself with the mathematicians try to persuade the mathematicians to do more work on the more general sorts of matrices that quantum mechanics was involved with?

Dirac:

No, I didn't, no.

Kuhn:

Did any of the Cambridge people take this up? I can't remember. One could tell that by looking at the published papers.

Dirac:

So far as I know, there was remarkable little follow up from mathematicians - even on the whole in Germany, where the ties were closer and where some of the mathematics that had been handled, particularly by Hubert and his group, was very close to some of the techniques. I think the mathematicians mostly go their own way, and they're not much influenced by physics. Although the Schrodinger theory of the hydrogen atom was the first example that was discovered of a linear operator with both discrete and continuous Eigen values. You knew that, did you?

Kuhn:

Yes. There is one other question that I simply wanted to check on when we stopped yesterday. I had raised with you the question of the likelihood that you were actually referring to de Brogue's formulation when you make the remark in the early paper that you can't utilize the notion of light quanta in thinking about the sorts of statistical problems you've been treating in the paper on detailed balancing.

Dirac:

I did know about de Brogue's work then; I had read some of it. Of course what I said was wrong, because I didn't then understand that one could have Bose statistic for particles with non-zero rest mass.

Kuhn:

I wonder whether at the point that paper was done, you knew Bose statistics at all?

Dirac:

No, no. That wasn't discovered until later.

Kuhn:

It's not much later; those two Einstein papers come out just at the end of 1924 and the beginning of '25.

Dirac:

Yes.

Kuhn:

But it wouldn't quite have been there. Did you see those papers immediately?

Dirac:

I think I did, yes, but I did not appreciate their importance.

Kuhn:

Was Fowler at all interested in them?

Dirac:

I think he was, yes.

Kuhn:

They fall right into his area of interest; on the other hand it's perfectly possible to say that this is nice mathematics, but just mad.

Dirac:

I had read Fermi's paper about the Fermi statistics and forgotten it completely.

Kuhn:

Had you?

Dirac:

Absolutely forgotten it, and when I wrote up my work on the antisymmetrical wave functions, I just didn't refer to it at all because I had completely forgotten it. Then Fermi wrote and told me, and I remembered that I had previously read about it.

Kuhn:

That's terribly interesting. Do you have an notion how that paper had seemed to you when you read it?

Dirac:

Well, I saw that it was the right statistics for electrons in an atom, but I didn't feel that it had wider applications. It was just a question of not appreciating a generality of the ideas.

Kuhn:

I'm very much interested both in the fact and in the way in which that sort of thing happens again and again I think.

Dirac:

It shows what a bad memory I have. If something doesn't strike me as being specially important, it's liable to slip out of my mind altogether.

Kuhn:

I take 'specially important' as fitting in to something that you're actively on at the time. Good. Well, then I'd like to go back more or less to the outline, and start by talking about your coming to Cambridge. You lived in St. John's when you came here?

Dirac:

I lived in lodgings. The first year I was in lodgings; the second year I was in College. The third year I was in lodgings again.

Kuhn:

What determines that?

Dirac:

Well, there are too many students and not enough rooms in college, so that as far as possible they're put in a college. Those who hold scholarships are

given precedence; they can always live in college. I had, an exhibition which didn't have the same standing as a scholarship. I think it was perhaps the general rule then for students to be living out the first year and in college the second year.

Kuhn:

What would then move you back out in the third year having gotten in in the first place?

Dirac:

I think there were just one in three living in college. Anyway Cunningham could tell you about these things because he was tutor for a very long time and dealt with these things. I met Cunningham when I first came to Cambridge for the purpose of the exam, in 1921. The only person I did meet I think.

Kuhn:

Living out, did you then live entirely by yourself? Did you eat in hall?

Dirac:

Yes, yes I did eat in hall every evening.

Kuhn:

Where did you work?

Dirac:

Sometimes in my lodgings, sometimes in libraries. My lodgings were often cold, and in cold weather I moved to a warm library. There were several libraries available. There were three: the Library of the Philosophical Society, the University Library, and the College Library.

Kuhn:

Does the Cavendish have a science library of its own in addition to these?

Dirac:

Oh yes, that would make four libraries available. Yes, the Cavendish had a little library of its own. And of course it wasn't nearly so crowded in those days as it is now. It was quite easy to sit undisturbed at a table for a whole morning.

Kuhn:

Do you have any recollection of how your time was distributed not between libraries, but between reading the literature, working on problems yourself, working up subjects that you were bearing lectures about, or actually going to lectures?

Dirac:

I went to certain courses of lectures, and did a good deal of reading on my own. I get more precise information from my own reading than from lectures. I don't seem to take it all in when I go to a lecture; I don't get the chance to refer back in the way I want to understand the thing properly. So I just get general ideas from lectures. The rest of the time I was just reading papers and thinking about them and trying to improve on them.

Kuhn:

When you read to work up a subject, do you regularly try, simultaneously, to work the things out yourself? I mean, do you read with a pencil and paper, and do at least go through some of the things?

Dirac:

If I really want to understand it thoroughly, I usually put it into my own notation, because other people's notation is usually not very suitable; it's unnecessarily awkward, and I like to put things in my own notation and get everything as simple as possible.

Kuhn:

Did you think you did that with most of the subjects you were listening to lectures on in that first year?

Dirac:

Probably not with most of them, but with the things that I was mostly interested in. The Hamiltonian thing I worked on a lot, by myself, and I used the Whittaker book also, Whittaker's Dynamics. In those days one thought that the action and angle variables were the all-important things. They come in to the Bohr Sommerfeld quantization. The question was to try to introduce action and angle variables for motion which was not periodic, or to introduce something corresponding to them.

Kuhn:

When you say here, ‘not periodic’ are you thinking of the generalization from periodic to multiply periodic?

Dirac

Yes.

Kuhn:

Did you worry at all in these years about doing something for the problems which were not even multiply periodic?

Dirac:

Well, I understood that they ought to be brought in, but I didn't know any way of doing it. I thought a great deal about it without any success.

Kuhn:

That sort of information, Sir, where it can be recaptured — you know, things that you were working on and didn't succeed with, but which seemed the real problems whether they got anywhere or not — is terribly useful, and I think

quite important. It's particularly important for this sort of work because it's just the information that, except where large amounts of notes are available, would not be forthcoming any other way. One must try to discover the shape of the problem structure of the field in the years just before the new quantum mechanics emerged.

Dirac:

I know I was very much impressed by action and angle variables. Far too much of the scope of my work was really there; it was much too limited. I see now that it was a mistake; just thinking of action and angle variables one would never have gotten on to the new mechanics. So without Heisenberg and Schrödinger I should never have done it by myself.

Kuhn:

You build action and angle variables, or something like them, also very deeply into your own early work in the new mechanics.

Dirac:

Yes, yes. Of course, I was then trying to fit in the new mechanics with my previous ideas of action and angle variables.

Kuhn:

Where does the term ‘uniformizing variables’ come from? You use this I think repeatedly, and I don’t think I’ve seen it elsewhere, but I may have.

Dirac:

I can’t remember; maybe Fowler used it in his lectures. I know I was very much under the influence of Fowler and used the same sort of language that he used.

Kuhn:

Did you see a great deal of him from the time you first arrived?

Dirac:

Yes, yes. He was officially my supervisor; that meant he was responsible for my work, and I’d go

and see him maybe once a week, or perhaps not every week. But roughly once a week.

Kuhn:

For how long would you be with him? And what would go on in these sessions?

Dirac:

Well, I'd just tell him about my work, and talk things over with him. I remember having one argument with him about something in the variation method, I didn't agree with what was put in the books, and Fowler thought that what was in the books was quite right, and he got a bit impatient with me. But I couldn't see his point; I think perhaps that early difficulty of mine was what led later on to constraints in the Hamiltonian theory. That was much later, you see.

Kuhn:

This is in quantum mechanical —

Dirac:

Well, it's really in classical mechanics, also. My generalization of Hamiltonian dynamics which I

worked on around 1949 and so on. It was very much later, but I think this was really straightening out that early difficulty.

Kuhn:

I don't know that piece of work at all. The older classical theory of constraints I once learned.

Dirac:

This is another kind of constraint.

Kuhn:

I would be glad for sort of a greater sense of the extent of Fowler's activity in guiding your reading and in talking with you about problems.

Dirac:

I think there was only that one occasion when I had a disagreement with him, and he got impatient with me.

Kuhn:

Did he generally follow quite closely what you were doing, or were these sessions more likely to be just 'checking in'?

Dirac:

I think he followed it and understood it, oh yes.

Kuhn:

Did he also, at the beginning, guide your reading a good deal, or did you pretty much pick that yourself?

Dirac

In the beginning he would tell me what to read. In one of your questions you asked whether I liked writing up papers; well, I didn't like it at all. When I first had any work to write up, and I told Fowler I didn't like writing up, he said, "Well, if you're not going to write your work up, you might as well shut up shop." He put it as definitely as that, and I knew it was just something that I had to force myself to do.

Kuhn:

Tell me more about writing up papers. Did. you do many drafts?

Dirac:

Not very many drafts. Words don't come to me very easily; I first have very rough notes, and perhaps two or three drafts, not an awfully lot. Not at all like what Bohr did.

Kuhn:

Is the paper pretty much done in your mind before you start to write it, or do you find other things in the course of writing, of trying to express it and react back and modify?

Dirac:

Well, I think it's pretty well done, and in the course of writing it up I may find I need serious alterations.

Kuhn:

Is it usual to find that serious alterations are needed?

Dirac:

Fairly often. I don't know whether it would be less than half, or not probably less than half,

Kuhn:

I know in my own work I've very often found that, but I've never been quite clear how common it was.

Dirac:

It's really quite irregular; one can't generalize about that.

Kuhn:

Do you show these drafts to people?

Dirac:

Not as a rule, no.

Kuhn:

So that you write it, rewrite it, rewrite it and then submit it?

Dirac:

Yes, yes. I don't actually rewrite it so much as make a lot of corrections in it. When I don't like writing, I try to cut that down to a minimum. I use an eraser very much.

Kuhn:

So it's a matter of a draft, many corrections, and then a final clean copy?

Dirac:

Yes, yes.

Kuhn:

I do notice, probably more in your papers than in any others that I have read recently — I think this is perhaps truest of your early papers a tendency, one or two papers later, to come back in the introductory section with a fairly substantial sketch of a method that has been laid out in an earlier paper, but which you're going to use again.

Dirac:

That is when the earlier method has been improved on. I think it is usually just when the earlier method has been improved upon. I think I've got an example of an early draft here. (Goes to look for it) It's in one of these papers I noticed

Kuhn:

I would have thought, and we may be able to explore this more clearly when the draft shows up, that in at least some of these cases, it really was not a matter of an improvement in the method, at least at the most technical level. The formulas might all be the same, but what seems to come through is a clearer and clearer exposition of what it's about, of why one does things this way, or just what is represented in

Dirac:

Very often it was just a question of putting the ideas in the right order, so that they can be best absorbed by the reader.

Kuhn:

When that happens do you think it was usually a matter of your own realization that the presentation might need clarification, or did people come to you and say, "I don't understand what you mean here"?

Dirac:

It was usually by myself, just thinking of a logical way of setting this thing down for someone who doesn't have previous knowledge of the subject.

Kuhn:

Were you from the beginning concerned deeply with clarity in the presentation; with the problem of the audience? People seem to vary tremendously in this respect; some people say, "If I get it down they should try to figure out what I mean."

Dirac:

I was more concerned with getting it clear in my own mind explaining it to other people was secondary to that, but when I did have it clear in my own mind then of course it wasn't so much trouble. It was just rather tedious to write the things down.

Kuhn:

Besides Fowler, were there other people that you saw with any regularity, either staff or students, in this first year or two in Cambridge?

Dirac:

Well, there were some that I saw frequently, but I wouldn't say regularly. There was Milne for instance; I went to lectures by him on astrophysical problems. Eddington I saw occasionally, but not very often.

Kuhn:

Did you see Milne outside of lecture as well as in?

Dirac:

Well, I did during the period when he was my supervisor, but I'm not sure about other times. There were the meetings of the various clubs where I would meet people.

Kuhn:

But on the whole, there was no one you think of as somebody with whom you really talked over problems, unless it was with your supervisor?

Dirac:

That would be the only person, yes.

Kuhn:

That would be equally true of other students?

Dirac:

Yes. I don't think it applies so much nowadays because so many people are working on closely related subjects, and they can get together very much.

Kuhn:

I get the impression that Fowler was really, until your own time, perhaps the only one here at Cambridge who was very much concerned with the problems of quantum physics and the only one from whom people would have learned about them. Is that fair?

Dirac:

Well, there was Rutherford on the experimental side. Rutherford and Fowler were a team; they dominated the whole field of quantum theory. Lennard-Jones to some extent - she was called Jones in those days; she became Lennard-Jones when she got married.

Kuhn:

One thing I raised in the questionnaire, but again a thing I would perhaps know better simply if I knew Cambridge or the British University system, is this for me rather unfamiliar division of what I think of as the physics curriculum into applied math and experimental physics, natural philosophy.

Dirac:

It's a division between those who do theoretical work and those who do experimental work. All those who do theoretical work are counted as in the mathematics faculty.

Kuhn:

Now, there is of course in the United States often an appreciable division between those who do theory and those who do experimental work, but it's rather in spite of the curriculum than because of it. They're in the same department, the examining requirements are likely to be much the same for the two, although there will be enough flexibility so that the subjects offered will often be different. What I wonder then really is, how separate were these two groups, how

much interaction was there with the experimentalists? How different was the preparation?

Dirac:

There was quite a lot of interaction during the days of Fowler and Rutherford. From the point of view of administration we had a close connection between pure and applied mathematics; we tried to keep students doing both pure and applied to quite a late stage and only specializing between them perhaps in the third year. In keeping pure and applied mathematics together it meant of course that the applied mathematics was more detached from physics. But Fowler and Rutherford were very close together. You know that Fowler married Rutherford's daughter?

Kuhn:

Oh yes. Did that closeness affect the students also?

Dirac:

Oh yes. I mean we would go to experimental colloquia: and there were the clubs which were divided between theory and experiment. They tried

to keep the “Del Squared V” fifty-fifty between theoretical and experimental people. Also there would be quite a lot of contact between theoretical and experimental students through these clubs.

Kuhn:

What about the colloquia at the Cavendish?

Dirac:

I always went to them.

Kuhn:

Would they be pretty much exclusively experimental?

Dirac:

Mainly experimental, not always; I have talked there sometimes myself. And Bohr has talked there; that's another meeting place for the theoretical and experimental physicists.

Kuhn:

... The impression I'm getting is that this administrative division between physics and mathematics did not prevent, in your period at least,

a quite close interrelationship between the experimentalists and the theoretical people.

Dirac:

No. I suppose the separation applied mainly to the undergraduates, because there would be a quite sharp division in the courses according to whether they were theoretical or experimental.

Kuhn:

Can you tell me more about what that would have amounted to? How much mathematics would somebody who was going to be an experimental physicist get?

Dirac:

Well, he took calculus and Maxwell's equations and I don't know where he would go beyond that.

Kuhn:

Would he have a mathematics examination?

Dirac:

I think he would, yes. I'm not very sure about this. I don't know whether you're asking about these old days, or the present.

Kuhn:

Well, on the whole I'm asking about the old days. You, I take it, did not take any Cambridge examinations as a research student, so that this issue of preparation for exams didn't arise with you.

Dirac:

No. It didn't arise with me at all. Of course I found that I knew less than if I had taken the Cambridge examinations; the mathematics examination in Cambridge was, well, a good deal more advanced than what I'd had in Bristol.

Kuhn:

Was there any particular thing that you discovered you were missing? Was it just that the level of the work was more advanced, or were there whole subject matters gone?

Dirac:

I think the level was more advanced, and there were whole subjects, like thermodynamics which were quite new to me. And then Gibbs statistical mechanics — that was quite new to me. There were whole subjects that were quite new.

Kuhn:

You have mentioned on more than one occasion having made great use of Sommerfeld's when you started to work with Fowler and learned about atomic theory and quantum mechanics.

Dirac:

Yes.

Kuhn:

Were there other things besides that that played a particular role? I wonder, for example, about Bohr's 1918 Quantum Theory of Line Spectra which was a hand book for some people? Or a little later -

Dirac:

I didn't study it very much, if at all.

Kuhn:

Do you remember whether you ever studied in any detail the Kramer's paper on the hydrogen atom which came immediately after that?

Dirac:

Where was that published?

Kuhn:

It is published in the Danish Academy Proceedings, as Bohr's was, I think just in the next year.

Dirac:

I don't remember it. I probably read most of the papers that were concerned with quantum theory; there were not nearly so many papers then as there are now. It was not so difficult to keep abreast of them.

Kuhn:

No, these were actually of course things that had come out before. These came out in 1918 and 1919.

Dirac:

Yes, well I had to go back to those.

Kuhn:

But you think of Sommerfeld's as having been perhaps the main book to which you returned and worked with?

Dirac:

That was the only book I think, and the rest was individual papers.

Kuhn:

Some of them get to be almost book length, that is, if you put the three Bohr parts together. What about Born's Atommechanik? This is a bit later; it's before you do your thesis.

Dirac:

I don't remember; was that already on quantum mechanics?

Kuhn:

It's one of the last book length works on the old quantum theory; in fact it's the book that he calls volume 1, saying at the beginning that it's so clear that this whole thing has to change that 'I hope to be able to write a volume 2 soon that will reformulate everything.'

Dirac:

I think it came out rather too late by the time I got to it; the new quantum theory had already appeared. I remember that book; I didn't really read it very much.

Kuhn:

I have been much concerned myself with trying to discover how different institutions differed in the period from '20 to '25, say, in their awareness that really fundamental changes were needed, and also in their consciousness, which also sometimes goes with this same distinction, of how pressing the old paradoxes were that existed almost since Planck or in some cases right since Planck. Such paradoxes as the conflict between classical theory and quantum

postulates, wave particle dualism which comes later, the sort of almost cosmologic contradiction on the one hand; and on the other hand there are those whole series of current problems that aren't quite being solved, like the helium atom, and the more general problem where you have the more general center of force. There were also more technical problems that may just work out, but at least in Copenhagen and Gottingen by 1923 or thereabouts people are quite clear that these things aren't going to work out without a basic reformulation.

Dirac:

I think they were not so clear in Cambridge. I was always thinking it would be in terms of action and angle variables.

Kuhn:

As we said yesterday, you are the first person to see and to state really clearly just where Heisenberg has broken with classical theory, pinning it right down to non-commuting variables.

Dirac:

Yes, well you told me that Heisenberg also appreciated that point very early.

Kuhn:

Well, he appreciated it as a problem; he thought that it shouldn't be • He was bothered by the fact that the variables didn't commute. You, on the contrary, point out that except at that one point, there is a full parallelism here and we can therefore take everything out of classical mechanics, break it at this one point — which of course means that there are certain sorts of classical derivations which we can't do the old way — but otherwise preserve a full parallelism.

Dirac:

I think there are many examples where the person who first introduces a new idea is bothered by something which doesn't agree with the old work, while other people just seize on it as the important thing. I can give one example of that which came later with the negative energy electrons. I felt right at the start that the negative energy electrons would

have the same rest mass as the ordinary electrons, and that bothered me very much. I felt that such positrons like that could not exist; otherwise the experimental people would have discovered them. That was my main worry at that time; I hoped that there was some lack of symmetry somewhere which would bring in the extra mass for the positively charged ones. Weyl was the first to point out quite definitely that the holes would have to have the same rest mass as the electrons. He wasn't bothered by it the way that I was.

Kuhn:

This is of course way ahead, but I don't want to let go of it. How early was that bother? I mean, I notice one thing particularly: the wave equation is '28; it's almost two years later before you come out with the proton theory, and I have no notion how, during the intervening two years, that problem has developed. You're quite explicit in the papers on the wave equation that the problem exists, not of course in terms of an expression in hole theory; but rather that there is this old problem of the negative energy levels which is a classical as well as a quantum mechanical problem, only you can't discard the

negative energy levels in quantum mechanics. In these papers, or this pair of papers, however, you are concerned only with the other part of the problem of relativistic theory. Then you just drop the question of the negative energy solution, so far as the published work is concerned, for almost two years.

Dirac:

It was an imperfection in the theory; it bothered me very much and I didn't see what could be done about it. It was only later that I got the idea of filling up all the negative energy states.

Kuhn:

Had you been coming back to that problem repeatedly over the intervening period?

Dirac:

Well, it always had been in my mind.

Kuhn:

But presumably the notion of filling up the other states and treating these solutions as holes didn't come to you until fairly shortly before you actually worked up the paper?

Dirac:

That is so, yes. Then I was bothered by the rest mass being the same.

Kuhn:

My impression is that in the actual paper, you don't really deal with that problem, with the lack of symmetry, in the 1930 paper.

Dirac:

Well, I think I say I hope that there is something which brings in a dissymmetry. I think I said that I hoped that the Coulombian interaction would bring in that dissymmetry. I felt then that if it did not bring in the dissymmetry, the whole theory would have to be counted as wrong.

Kuhn:

Well, now let me bring you back to this question that we were getting at when I raised the question of isolation — the nature of Heisenberg's break. Also, I think you felt that he was preserving much more than I think he realized he was preserving. Your whole clarification, from the very start, of the

relation between Heisenberg's variables and the classical variables I think makes it look vastly more classical than it has in Heisenberg, and simultaneously isolates cleanly the single point at which you say that he has broken with the classical thing. I raised this because I wanted to ask you to what extent all that you had been doing and thinking about yourself, had prepared you for a break of this sort. Not that particular break, but for something that was as unclassical in a sense, or unHamiltonian.

Dirac:

I think it hadn't prepared me at all, and it was quite a surprise to me. I could say something in further elaboration of the previous point. A person first gets a new idea and he wonders very much whether this idea will be right or wrong. He is very anxious about it, and any feature in the new idea which differs from the old established ideas is a source of anxiety to him. Whereas someone else who hears about this work and takes it up doesn't have the same anxiety, an anxiety to preserve the correctness of the basic idea at all costs, and without having this anxiety he is not so disturbed by the contradiction and is able to

face up to it and see what it really means. I expect that was just Heisenberg's problem. He was afraid that this lack of commutation might cause the whole theory to collapse; he was probably terribly worried about that. That rather stops him from really facing up to it.

Kuhn:

I hope you will continue to say things of this sort also as we go, because they're immensely helpful.

Dirac:

I think that applies quite generally to all new ideas which are brought in by anybody.

Kuhn:

Can you think of other cases of that sort, either involving yourself, or involving others?

Dirac:

I expect if I thought about it, I would remember some examples. They don't occur to my mind immediately.

Kuhn:

It would be terribly illuminating to pick up more examples of this [sort of thing] from this period, and particularly from your own work if you think of them, on the same point of the sense of crisis and the sense of the need for a break from Copenhagen in particular, but also to some extent from Göttingen in the period from '23 to '25.

Dirac:

Well, I do think of one example, namely, the relativistic theory of the electron. When I first got that equation, of course I was very anxious to know whether it would work for the hydrogen atom, and I just tried it by an approximation method. I thought that if I got it anywhere near right with an approximation method, I would be very happy about that. It needed someone else, namely Darwin, to tackle that equation as an exact equation and see what the exact solutions were; I think I would have been too scared myself to consider it exactly. I would be too scared that it would get unfortunate results which would compel the whole theory to be abandoned.

Kuhn:

That's fascinating; does this mean that you had yourself not tried to handle it exactly before going to an approximation method?

Dirac:

That is so, yes.

Kuhn:

You looked for the approximation method from the start?

Dirac:

Yes, yes. Of course I had the fear that the whole theory was nowhere near right, and if I could get it approximately right, well, my confidence would already be substantially increased in that way. It's just that one has lack of confidence when one introduces something quite new.

Kuhn:

That's terribly interesting. I will ask you at this point a question that I had intended, to ask later. When you got the equation, were you looking for an

equation which would give spin, or were you looking for a relativistic equation?

Dirac:

I was looking for a relativistic equation.

Kuhn:

When you did the approximate solution, was what you were hoping to approximate something with or without spin? I mean was it a surprise that what came out were spin terms?

Dirac:

No, I don't think so, because one had the Pauli matrices in it. I remember when I was in Copenhagen quite early Bohr asked me what I was working on, and I told him I was trying to get the relativistic theory of the electron. And Bohr said, "But Klein has already solved that problem." I was rather perturbed by that, but Bohr seemed to be quite complacent and satisfied. with Klein's solution, and I wasn't. I remember it disturbed me quite a lot that Bohr was so satisfied with it because of the negative probabilities that it led to. I just kept on with it,

Kuhn:

At what point did your insistence on a linear equation and the preservation of transformation theory enter your work on that problem?

Dirac:

Well, previously the transformation theory had been set up in a general form and I felt that that was correct and had to be preserved, and had to be fitted in with relativity.

Kuhn:

So that was really the basic guideline of the attempt, to get a relativistic formula from the beginning?

Dirac:

Yes. It was just that I had confidence in the transformation theory of quantum mechanics. I suppose one's research is guided very much by what one has confidence in and what one is feeling doubtful about. But I suppose I had more confidence in that transformation theory than other people did, I felt it imperative to keep to positive probabilities.

Kuhn:

Either someone told us or in one of the accounts of things going on in Copenhagen it is written down that the problem that you raise at the beginning of the paper on the wave equation, “Why isn’t Nature satisfied with a point particle, why spin?” was also very much on your mind in Copenhagen. Possibly not on your first trip to Copenhagen, but it is said that at least it was a problem that you had raised and discussed there.

Dirac:

Not to the same extent. The main thing was to extend the transformation theory of quantum mechanics.

Kuhn:

Do you remember at what point you suddenly saw spin coming out — saw that the extra variables that you were introducing?

Dirac:

Well, one had it non-relativistically before then. It came out just from playing with the equations rather

than trying to introduce the right physical ideas. A great deal of my work is just playing with equations and see in what they give. Second quantization I know came out from playing with equations. I don't suppose that applies so much to other physicists; I think it's a peculiarity of myself that I like to play about with equations, just looking for beautiful mathematical relations which maybe don't have any physical meaning at all. Sometimes they do.

Kuhn:

Well, now, in the case of the wave equation, what do you suppose you were here playing with - a general linear form?

Dirac:

Yes, yes.

Kuhn:

And it's somewhere then in the manipulation of the alphas that you begin to discover spin, that you can do things with the Pauli spin matrices, or that you get conditions that look, in some cases, like the Pauli spin matrix conditions?

Dirac:

I remember noticing that just forming the three dimensional scalar product of sigma with the momentum gives you something which is quite nice to play with, and I wanted to extend that to four dimensions. And of course one just can't do it sticking to the two by two matrices, and it needed quite an effort to make the further generalization to the four by four matrices. But that work did come from playing about with the three dimensional scalar product and trying to extend it.

Kuhn:

How long do you suppose that went on?

Dirac:

Maybe well, it was a matter of weeks, not more. I don't know whether you knew that Kramer's had independently discovered the second order wave equation which you can get by multiplying up my first order wave equation. He told me afterwards that that second order equation he had found himself. I don't know how he found it, but just by trying to bring the spin into relativistic theory, I guess.

Kuhn:

No, I had no idea of that. That I take it was not published?

Dirac:

No, it was not published. I suppose he was just working on that sort of question when my paper came out, and then it was not necessary for him to continue that line of work.

Kuhn:

As I understand this line of development you were not particularly working for spin, but that in fooling around with the equations, playing with the linear form, you began to see things like the Pauli spin matrices and began to see that spin might, and ultimately was, going to come out of this equation into which nothing of the sort had gone in. My question then really is, can you remember what it felt like, to see this really quite unanticipated result two puzzles tying together this way?

Dirac:

Well, in the first place it leads to great anxiety as to whether it's going to be correct or not. That anxiety that I told you about before. I expect that's the dominating feeling. It gets to be rather a fever; you work things out with it and see whether it's going to turn out right or not.

Kuhn:

When that fever begins to develop and the anxiety builds up, do your working habits change accordingly? Do you find yourself working much longer hours? Do you have trouble sleeping?. Do you take more walks, or fewer walks?

Dirac:

Well, I take less interest in the outside world. But the excitement does prevent one from working things out in an ordered way. If one was not so emotionally excited by it, one could get on faster.

Kuhn:

In the '23 to '25 period, as you yourself were getting quite deeply involved with quantum mechanics,

there's a lot of work going on in Copenhagen and secondarily in Gottingen on extensions of the old quantum mechanics the whole notion of the non-mechanical forces and the attachment of two quantum numbers to coupling. The Bohr-Kramer's-Slater paper, and the non-conservation of energy in the virtual oscillators, the Kramer's dispersion formula, are all products of it. And of course, Heisenberg's paper, in a sense, is also a product of this great broadening out of classical Bohr quantum mechanics. I wondered how much of that work was really known and followed closely. And this is work done by people who are pretty clear that there is going to have to be a major change in a way that, I take it from what you say, you were not really clear it would have to change.

Dirac:

I think that is so, yes.

Kuhn:

Were you much aware of that work? There's a paper of Heisenberg's with three different ways of —

Dirac:

I was aware of the general ideas, but I didn't follow all the details.

Kuhn:

Were you at all sympathetic to them?

Dirac:

Well, when the Bohr-Kramer's-Slater idea came out I thought probably it was right, to begin with. I tried to adjust myself to it, but of course it didn't survive very long.

Kuhn:

Well, the non-conservation of energy' didn't survive very long; the virtual oscillator approach, though, goes straight into the Kramer's' dispersion formula and the Kramer's-Heisenberg paper and into a number of other broadenings of the Correspondence Principle. And in that sense it goes into Heisenberg's matrix mechanics without matrices' paper.

Dirac:

I don't think I was very strongly influenced by this work. I have a general view that it is best to let people work out their own ideas.

Kuhn:

What about the Kramer's dispersion formula?

Dirac:

Well, I was interested in it, but I didn't see how it could go on from that. When one reads about work like this the question arises, "Is it a starting point from which one can go on?" and I didn't see how to go on from that dispersion paper. But from the Heisenberg matrix paper, well, I did see how to go on from that.

Kuhn:

In your own paper, in which you go on from the Heisenberg paper, you refer to the Kramer's-Heisenberg dispersion paper as giving an example of a case in which the derivative of one q -number with respect to another comes out in a form that can be looked upon as a commutation form.

Dirac:

Well, I don't remember that. Do you want me to refer to it?

Kuhn:

Yes. This is the paper on the fundamental equations in quantum mechanics. [Paper No. 8] [Dirac searches for the paper, reading some other titles]

Dirac:

Here's that —. That's this "Adiabatic Invariance of the Quantum Integrals." [No. 5] I think that must be Fowler's comment up there. [referring apparently to marginalia]

Kuhn:

This is really an extremely condensed version isn't it, of the —?

Dirac:

Yes, I've quite forgotten what it is; I haven't read it (for a long time).

Kuhn:

Well, actually I notice that this is an outline of the method. ‘The first two formulas that you introduce are actually numbered and 6. Have you many drafts that go back to this period?’

Dirac:

I didn’t know I had any; I just happened to see that the other day. There might be a few more if I searched.

Kuhn:

I was thinking particularly of this paragraph here which goes back to - well, your revision goes back to 9, I see, that actually makes a substantial difference. It may throw off my whole question.

Dirac:

What was the question?

Kuhn:

Well, in the printed work here, this is the reference to the Kramer’s-Heisenberg theory. [i.e. in connection with equation 8: $dx/dV = x_a - ax$]

Dirac:

There is a misprint; I wonder if that helps any.

Kuhn:

Well, it surely does, because 8 was this, which is a very fundamental relationship. I didn't in fact understand, but I haven't gone back and tried to piece together a way in which one might put the Kramer's-Heisenberg formula that way. If it's 9 it may still be a question, but it becomes a different question.

Dirac:

Yes, I suppose there is some comment which probably is made later on.

Kuhn:

I might, having gotten this out, ask you one other question. I may be missing a point in the nature of the argument in this paper, but I'm perplexed here about the way in which the whole question of the derivative of one q-number with respect to another enters in this paper • The third section is called quantum differentiation, and comes very neatly to

the conclusion. that it's got to take the form of a commutation relation. On the other hand, that's the only place in which I can see that the question of differentiation really enters here. You could, I think, have gone right on to the question of the quantum conditions in paragraph 1 and right through the rest of the paper without ever explicitly introducing quantum differentiation at all. That suggests to me that it plays an essential role in the way you get to this.

Dirac:

Yes, I think I was looking for something to replace the partial differential equations of the Hamiltonian theory.

Kuhn:

And perhaps you were looking for that before you thought of writing them in the Poisson bracket form directly?

Dirac:

Yes. I know I was working for quite a long while on trying to get a connection between the Heisenberg

formulas and the action and angle integrals, and the Hamiltonian equations of motion.

Kuhn:

And this was written not in the form of Poisson brackets, but in the more usual partial differential form?

Dirac:

Yes, yes. In the usual differential form.

Kuhn:

That would exactly answer the question.

Dirac:

I didn't know much about Poisson brackets at that time. Did I tell you how I first came to think of the Poisson brackets?

Kuhn:

No. You told me in part when, but I'm very much interested.

Dirac:

I used to take long walks on Sundays and get away from the work altogether, and at the end of those walks I would perhaps go on with my work a bit in a refreshed state of mind. And after one of these Sunday walks it occurred to me that the commutator might be the analogue of the Poisson bracket, but I didn't know very well what a Poisson bracket was then. I had just read a bit about it and forgotten most of what I had read, and I wanted to check up on this idea, but I couldn't do it because I didn't have any book at home which gave Poisson brackets and all the libraries were closed. So I just had to wait impatiently until Monday morning when the libraries were open to go and check up on what Poisson brackets really were. Then I found that they did fit, but I had one impatient night of waiting.

Kuhn:

I'm wondering about the preparation before this walk. You said when Fowler had first given you the proof of Heisenberg's paper, you had looked at it and hadn't thought it amounted to much.

Dirac:

That is so, yes. I don't remember what my earlier reaction was; I've often tried to recall it, but I can't remember what it was. I supposed it was just some disparity between that and the Hamiltonian formalism. I was so impressed then with the need of Hamiltonian formalism as the basis of atomic physics, and anything that didn't connect with it I thought wouldn't be much good. I expect it was something on those lines, but I don't remember just what it was.

Kuhn:

Do you have any notion what it is that brought you back to look at it again, or what it was that you began to see in it when you began to pay more attention to it? I take it, namely, that there is a stage that intervenes between this putting it aside and the decisive walk, when it occurred to you that the Poisson bracket might be the analogue; but you've been working quite hard. in the attempt to bring it into a Hamiltonian form meanwhile.

Dirac:

I first got the paper in September, 1925, the beginning of September, and I think it was perhaps the middle of September when I went back to it again. I suppose I realized that it was introducing the new idea of the non-commutation. It was already in September, 1925, that I realized that this paper did, mean a breakthrough.

Kuhn:

You don't really remember what it was about it that suddenly made it seem more important, or that convinced you that perhaps this was a breakthrough?

Dirac:

No, I don't. In fact it seemed pretty obvious then, and I really find it more difficult to understand why I didn't see it the first time I looked at this paper. But I then worked quite hard to try and connect these matrices with the action and angle variables, and that took a good many weeks, maybe a month or two, before this walk occurred in which I got the idea of the Poisson bracket.

Kuhn:

Did you try to get any applications into the first paper? That is the first paper cuts off very sharply with just a presentation of the formalism, if you will.

Dirac:

I expect what happened was that I showed it to Fowler, and he was very much impressed by it and saw the need to publish it urgently. He told the Royal Society to give priority to my paper, and they published it extremely quickly. I expect you notice that with these early papers of mine there's only a very short interval of time between when the paper was sent in and when it was published. Well, I can thank Fowler for that, because he appreciated that it was urgent, and I suppose he was thinking that there would be competition from other places. I expect Fowler told me that I ought to publish what I had, and then go on to further papers from there.

Kuhn:

In the papers that immediately follow this one, particularly then the work on the hydrogen atom and then on the more general multi-electron atom, you

effect a gigantic versatility with q -numbers. You know, one can follow your arguments, but one can't imagine having invented them. How easy did that —

Dirac:

I think it came pretty easily; I can't remember having any special difficulty with it. The difficulty was with the physical interpretation, and I think it took a year or two to get that straightened out.

Kuhn:

But the problems of the approximations and finding the transformations and so on, came fairly easily and directly? There is some awfully elaborate q -number algebra, or q -number geometry - we'll leave open the question of which — in the paper on the elimination of the nodes. [No.10) I've read through them, but in those cases I certainly have not tried to go through the entire -

Dirac:

I'd call it algebra; I could settle down to algebra when I had the basic ideas given, but to get new basic ideas I worked geometrically. I think perhaps that clears up that discrepancy. For getting new ideas

I worked geometrically; once the ideas are established, one can put them in algebraic form and one can proceed to deduce their consequences. That's just a question of algebra. The more difficult part, and the more important part, is the getting of the new ideas, and that requires a geometrical mind, I believe. I suppose that it's to some extent the geometrical mind when you think that q -numbers can be used with a great deal of analogy to ordinary numbers. I suppose that was the main point in my early work, that I did appreciate that there would be a very close analogy between the q -numbers and ordinary numbers.

Kuhn:

Your approach through q -numbers really, from the first paper after Heisenberg's on, is, as you yourself point out, rather different from the Heisenberg-Born Jordan approach through representations and through the matrices, taking the matrices to be themselves fundamental.

Dirac:

Yes • I see now that that comes just from realizing that the most important thing is the non-

commutation, just the thing which disturbed Heisenberg.

Kuhn:

But here, on that point also, Born-Jordan are also quite explicit and pull the non-commutation out as at least also their form of the —

Dirac:

They don't have the same idea as Heisenberg would have about it.

Kuhn:

You pushed for some time, and with immense, fruitfulness, a rather different way of setting up and getting at quantum mechanics from the matrix mechanics way.

Dirac:

Yes, What one might call a symbolic way.

Kuhn:

With representations to be secondary. Certainly to me it has that sense of being a more fundamental approach. It also raises some problems, since you've

still got the representation to get out when you've this.

Dirac:

Yes.

Kuhn:

I wonder what sort of problems this may have made at the time, for you, and for other people trying to read the works and put the various pieces together. Did anybody ever urge you to get on the matrix bandwagon?

Dirac:

No, no. Fowler was certainly quite happy that I was getting on my own wagon. He certainly wouldn't urge me to change my method at all.

Kuhn:

Was it ever tempting? Did you ever think seriously —

Dirac:

No if you've got a good method, you stick to it. And anyway my method gave results quickly, without

requiring very much writing; I'm a (lazy) person who doesn't like to write things up.

Kuhn:

If one can do that much in one's head. Wouldn't it be fair to say that an awful lot of other people have been able, with great labor, to get results out of, let's say, the matrix method, which is in a sense fairly straight-forward. You can write it all down, hold all the indices, finally solve all the differential equations. But there isn't any sort of parallel mechanical way to do the q-number manipulations, so that if you can't see them in your head and therefore do them quickly, you're going to have great trouble doing them at all?

Dirac:

I don't think that it's quite right to say that I see them in my head; I have to play about a good deal with these q-numbers and see what relations they give. I find that some of these relations are useful, and I try to get the useful relations in the simplest possible way, and then I publish that • And all the intermediate steps get scrapped. So it wasn't a question of finding out these methods straightaway.

They were often obtained only by very circuitous —. I have a sort of recollection that the work that I did on the hydrogen atom with the spinning electron equation was very much on those lines. And the method for handling the angle variables in that problem I put in the published paper in the simplest possible form, but that was a very different form from the one in which I first worked it out. You must first work it out and get the results and then labor on that quite a lot until you see how to get the form in the most direct way. This kind of procedure is what is done by Marcel Rises quite a lot; I don't know whether you know him.

Kuhn:

No.

Dirac:

He's a pure mathematician; he's really very much concerned with putting his results in a tidy and beautiful form, and he really considers that the main part of his work is not to obtain the results, but after you've obtained them to find out the neatest and most direct way of getting the results. I didn't meet Rises until very many years later, but I had been

following the same sort of method he follows. He worked for a long time in Lund, but he has retired now.

Kuhn:

You said before that the algebra came fairly easily, that what was tough and took a long time was the interpretation. Were you concerned with the interpretation from the very beginning?

Dirac:

Oh, yes, yes; it's a physical theory, so one had. to have some interpretation for the q -numbers.

Kuhn:

In talking to Heisenberg and to a lesser extent talking with Born, I raised this same question. You know that whole series of matrix mechanics papers is notably free from any questions about, "What's this all about and how are we to understand it physically?"

Dirac:

Well, I shouldn't have thought there was that problem in the matrix formulation, because you have the matrix elements with their direct application.

Kuhn:

That does answer it. It's a much more minimal sort of answer, isn't it, than the one that emerges, but I see how it stops the question.

Dirac:

The whole idea of the matrices was to work entirely with numbers which have a physical interpretation.

Kuhn:

It can leave all sorts of questions which emerge immediately. The problem that you're faced with with the q-number which has not yet been represented by a matrix is a question that you can also be faced with by the matrix if you ask not, 'what does each of these elements mean?' but 'what does the array mean — why are they all there?'

Dirac:

Yes, you don't have any problem with the interpretation then.

Kuhn:

No.

Interview Session – 4

Kuhn:

I'd really like to go back once more to your early times at Cambridge and ask you a little bit more about the people. I wondered particularly, was J.J. Thomson still any sort of a force of importance?

Dirac:

He was still about, yes, yes.

Kuhn:

Did one see anything of him and did he still exert any real influence on the direction

Dirac:

I saw him occasionally in the lab. I think probably Cunningham could answer such questions better than I could. ... I can't remember Thomson's ever giving a colloquium while I was there. He might have given one which I've forgotten completely. He kept a room in the Cavendish and I think he went there occasionally, but not very often.

Kuhn:

I know in one of the few conversations we've had with Professor Bohr he spoke of Thomson's reaction to the Bohr atom and of course then his reaction on the whole subject of isotopes and spoke of him as really having I don't remember his words but they were to the effect that he had really departed from physics at that point. That by his refusal to keep up and his strenuous opposition to what for so many people were critical new ideas, he really ceased to be a member of the profession and I wondered whether there, was any sense of that.

Dirac:

What time was that? What date was that?

Kuhn:

Well, this would have been by 1920, in any case, I think.

Dirac:

I don't remember his coming to any colloquium and taking part in it.

Kuhn:

Can you say any more about Fowler than you already have? Was he not only a person who initiated you, I take it, to quantum mechanics, but was be doing this for many people?

Dirac:

Yes, he was. Yes.

Kuhn:

Was there a real group around him?

Dirac:

There was a group, a small group. The groups then were very much smaller than they are now. But he 'was a great stimulating influence. He was really the center of the quantum theory in Cambridge.

Kuhn:

What form did this stimulus take or how was it exerted? Was it strictly in his lectures?

Dirac:

Well, for one thing he was so excited about it and that excitement was infectious. He went to Copenhagen pretty often and came back and reported at colloquia what he had heard and just generally one caught this excitement from him.

Kuhn:

And this was principally in colloquia and in lectures less, I take it in face to-face encounters.

Dirac:

I think also then, yes.

Kuhn:

What sort of a person was he?

Dirac:

Well, he was tall and athletic • He was very vigorous and healthy looking and it was quite a surprise when he died so young. Well, there's a picture of him at one of these Solvay Conferences. [showing picture] He was present at this conference here.

Kuhn:

What about Rutherford as a figure here at that point? Clearly he was the leader of major experimental efforts still.

Dirac:

Well, Rutherford dominated the Cavendish. Fowler dominated the theoretical side.

Kuhn:

Did Rutherford have any particular effect on the theoretical side?

Dirac:

I don't think he did, no.

Kuhn:

Did he follow it?

Dirac:

Well, he wasn't much of a mathematician. His mathematics was enough for him to deduce the Rutherford scattering law but not to go further than

that. Of course, he appreciated mathematics, but he just didn't tend to follow it himself.

Kuhn:

But he must have heard a good deal from Fowler as to what was going on.

Dirac:

Oh, yes. They were very close.

Kuhn:

Do you have any notion how he responded to these drastic changes that were taking place at the time?

Dirac:

Well, he accepted them anyway. He was mainly concerned with experiments.

Kuhn:

Aside from Eddington, of whom you've already spoken, is there anyone else in the group who had a particular role for the theoreticians or for the experimentalists in sort of shaping the direction of research?

Dirac:

Well, Milne had some role. He was mainly concerned with astrophysics questions about opacity of gases did come into that and of course he was interested in what theory had to say about opacity of gases. So Milne was concerned with it.

Kuhn:

What about people who were students at the time you were studying I don't want to make that too exact a coincidence of dates but —

Dirac:

I don't remember any of them having much influence. One fellow student (at John's) called (Schlapp) who is now professor in Edinburgh, and he had Larmor for his supervisor. Larmor was, of course, interested only in the old classical ideas. He set (Schlapp) to work on classical problems so he was rather away from quantum theory for that reason.

Kuhn:

I realize you' we spoken of the accidents involved in your getting Fowler as your supervisor, and your rather hoping you'd have Cunningham.

Dirac:

Yes, but I soon saw that Fowler was more suitable than Cunningham.

Kuhn:

No, I take it that this was at the very least a very fortunate chance which brought you to him; to what extent could a student coming here make a choice himself in the supervisor? To what extent would he be simply given somebody; would that have a more or less permanent effect on the work he did?

Dirac:

It is the degree committee of the board of mathematics which decides who the supervisors are for each research student.

Kuhn:

The student has no choice himself?

Dirac:

Well, he can say he would prefer to work with somebody. I don't think they usually do. I think they just say what subject they want to work on and then the degree committee has to find someone who is willing to work on that subject with the student.

Kuhn:

But they do specify subjects so that at least they would not get off into classical problems if they wanted to do quantum mechanical problems.

Dirac:

They do specify the subject, yes. But usually they don't know very much about what they want to do and they're rather vague about the subject • After all, when a student starts, he doesn't really know what scope there is in different subjects.

Kuhn:

Do you have any notion what proportion of the students at your time were doing classical and what proportion were doing problems that were closely

related to the area of quantum mechanics or relativity?

Dirac:

I don't remember. One can look that up by seeing what the well, one can look up the Ph.D. theses of the different students and check up on that. There is a publication giving the summaries of the Ph.D. theses.

Kuhn:

Coming back now to these early papers, the ones before Heisenberg of which we've talked of a good many, there are just a few questions that I still wanted to ask. I think I mentioned in the outline looking through that group of papers I think seven in all of which only the first, the one on dissociation under a temperature gradient, doesn't quite fit the pattern I wanted to point to. All of the other six papers have in common something which I don't see in the same way in your later work; that is, in each of these cases in one way or another you're taking a topic that's relatively standard in the literature and criticizing existing results and doing them, putting them on a firmer basis than before.

Dirac:

Well, that's how I got subjects for research. I was reading quite a bit, and well, every now and again I saw a chance of improving on something which I had read and that led to a paper. Of course, it was really the same with Heisenberg's first paper. I saw a chance for improving on that, by concentrating on the non-commutation.

Kuhn:

I suppose so, but the whole feeling of that paper is very different —.

Dirac:

You can say that it was all built up from Heisenberg's paper in the same way the previous ones were built up from other papers.

Kuhn:

I guess I could; I wouldn't, in the sense that this goes, in a way, so much further —.

Dirac:

But I started out in the same way. Let's put it like that.

Kuhn:

Was that a sort of work that you found very satisfying or did you view these as parts of education?

Dirac:

Well, it was really the only kind of work I knew at that time. Of course, the difference with that first paper, "Dissociation under a Temperature Gradient" was that I hadn't done any reading before that. That was the first subject that was set to me while the later ones did depend on my reading.

Kuhn:

You had a larger role in finding them.

Dirac:

Yes.

Kuhn:

I started, I realize, two or three times to ask you the question and I've never finished the sentence for some reason or other but in the paper on "The Doppler Effect [Principle) and the Bohr [Frequency) Condition" [No. 3) in which you produce a relativistically invariant formulation by treating the energy change from the phase function —

Dirac:

Yes.

Kuhn:

I've brought this up before in connection with de Broglie's work because he does and I can't remember now whether he does it in the piece in the Phil. Mag. or not or whether he does it just a little bit later. He does things very much like that four dimensional formulation of yours, not for the same problems but in the same area. I think he does the identical thing with the phase function. I wondered whether there was any likelihood that you had seen things in his work that were carried over there or whether that idea started from scratch.

Dirac:

I just don't remember. Was de Broglie's paper published sometime before mine?

Kuhn:

It's published nearly at the same time.

Dirac:

One would have to check up on the dates.

Kuhn:

It's only very late, in the late stages before the thesis that de Broglie in his publications begins to do this sort of four dimensional formulation.

Dirac:

Yes, I don't remember being influenced by de Broglie in writing that little paper.

Kuhn:

I'm also curious in looking at those early papers as to what determined the place one would choose to publish a paper. Those papers come out variously in the Philosophical Transactions, the Cambridge

Philosophical Society Proceedings one of them in the Phil. Mag.

Dirac:

There were none of them in the Transactions. I think the more important ones were sent to the Royal Society Proceedings. I think Fowler suggested where I should send them to.

Kuhn:

What besides the importance would be a determining factor?

Dirac:

A very short note would hardly be a suitable paper for the Royal Society [Proceedings].

Kuhn:

But what about there's the Phil. Mag., Monthly Notices of the Royal Astronomical Society, and the Proceedings of the Cambridge Philosophical Society.

Dirac:

Well, the Astronomical Society would only take papers which were of astronomical interest. They are not general in the way the others are.

Kuhn:

What about the Proceedings of the Cambridge Philosophical? Was there any sense that these were likely to get lost for a larger audience? I realize I have very little notion how widely that journal was read.

Dirac:

It was read pretty widely, yes. It has quite a high standing. It certainly did at that time, perhaps not quite so much now because the *cal Review* now dominates the situation. But it did have quite a wide standing.

Kuhn:

So one didn't have the sense that something placed there was likely to be lost.

Dirac:

No, oh, no. It certainly wouldn't be lost. I think all the libraries have it. I haven't come across any physics library which doesn't have it.

Kuhn:

No, I think that would be quite true, except perhaps for the library in Rome, which I discovered was in this period lacking in several journals, including *Naturwissenschaften*.

Dirac:

Yes.

Kuhn:

But I think you're quite right that it would be in many libraries. Of course, I also notice that one can't count in this period on any given physicist's having followed all of the journals that the library would have.

Dirac:

Yes.

Kuhn:

You would almost certainly have followed the Royal Society and possibly the Phil. Mag.

Dirac:

It was of higher standing to get a paper published by the Royal Society, but in any case it would be widely read.

Kuhn:

When papers come in either to the Royal Society or to the Phil. Society here they're always communicated by a member.

Dirac:

By a Fellow of the Society, yes.

Kuhn:

Is it permissible to assume that the person who has communicated the paper has really been over it fairly carefully? Does he take any real responsibility for it?

Dirac:

He should take some responsibility for it. I would, if I were sent one. I wouldn't follow all the details, but in any case I'd see that it wasn't a crank. I'd see if the main ideas are worth writing about, even if I don't check to see if the conclusions are all correct.

Kuhn:

Do you think you were perhaps better than most about this? I ask this only because there are some interesting patterns, I asked Louis de Broglie about such endorsements and he immediately assured me that so far as transmission for publication in the Comptes Rendus was concerned, almost nobody really looked at those papers or at the papers he communicated if they came from, or through, somebody he knew. There was apparently no reason for presumption of some real responsibility toward the subject matter being taken by the person who presented the paper. Was it as loose as that in England?

Dirac:

Well, the person who communicates the paper does have some responsibility, but there is also a referee. He doesn't have the sole responsibility.

Kuhn:

Was that true for all of the journals; that is, true for the Royal Society also for the-

Dirac:

Cambridge Society. But for the Philosophical Magazine anyone can communicate a paper. I mean the author can send in his own paper without having to get someone to communicate it.

Kuhn:

But there will always be a referee in addition to the person that communicates it for all of these.

Dirac:

Yes.

Kuhn:

We've talked scarcely at all about the paper on detailed balancing. I'm a little unclear as to what to ask about it except that it's a particularly interesting paper. It is the first one that gets sent to the Royal Society. How did you come on that problem?

Dirac:

Well, Fowler was interested in this question of detailed balancing. It was an important question of statistical mechanics at that time. People had thought of this, I don't remember who thought of it first and it did enable one to get information.

Kuhn:

I think probably Klein and Rosseland, are the first people who at least show it in the sort of application.

Dirac:

Well, Fowler had taken it up and was using it,

Kuhn:

How much did he work with you on that? Was he a more active participant in that paper than in the others?

Dirac:

I think I mainly worked it out by myself and then showed him a more or less finished version.

Kuhn:

Were there any particular reactions to it or follow-ups on it?

Dirac:

I don't remember any.

Kuhn:

Or with respect to the pair of papers on the adiabatic principle that you do? One of them I think for the Royal Society and the other one in the Cambridge Proceedings.

Dirac:

Yes, I've forgotten that one in the Cambridge Proceedings.

Kuhn:

It's a very short one that deals particularly with the case of magnetic fields, velocity dependent potentials. It discusses the treatment, corrects and justifies the result, among other things, of a way of treating magnetic fields that Sommerfeld had used in Atombau. And those, I take it, do come out of your reading.

Dirac:

Yes.

Kuhn:

I'm particularly interested in the first of those papers in which you develop a more general and more powerful technique for evaluating invariants through points of frequency degeneracy

Dirac:

Is this the one in the Royal Society?

Kuhn:

Yes. And there you build it entirely, so far as your citations are concerned on one early paper of Burgers.

Dirac:

Yes.

Kuhn:

In the Proceedings of the Amsterdam Academy, I think. My impression is, although I don't know that literature very well, that there'd been a good deal else that had happened in the interim.

Dirac:

Some other people have written about it, I remember that someone writing a summary of it for one of these magazines' which give summaries of scientific papers, had written unfavorably about my paper and said that. some other paper was better and no one would read mine. I thought that was an unfair comment on my paper and the person who wrote that summary hadn't appreciated the difference between mine and the other work, namely that in my

paper one could check whether the conditions are fulfilled or not without having to integrate the adiabatic equations, while in the other paper one would have to integrate the adiabatic equations. It was the only time I have been dissatisfied with the comments which someone has made on my work,

Kuhn:

Had you known the other paper? The one that he felt was better —.

Dirac:

I don't think it came out before mine. I think it was independent. I'm not sure of that.

Kuhn:

Well, if it was independent and didn't come out before yours, it's a rather odd sort of criticism.

Dirac:

Well, he just said the other paper was better and that the reader needn't bother to read my paper.

Kuhn:

Did you or anyone else apply the technique you there developed?

Dirac:

No. Well, we very soon lost interest in it because of the appearance of the new theory. If a new theory hadn't come out we would probably have worked more on it.

Kuhn:

In that paper there is a very intriguing remark which I've never tried to follow up in my own mind to see how it would work, that there is a sense in which you can reduce all problems involving multiply periodic systems to the two-dimensional case, because of the selection rule.

Dirac:

I don't remember that remark. Might be wrong.

Kuhn:

I'm not sure whether I can say enough more about to bring it back.

Dirac:

Well, it's best to look up the paper then. [Begins to search for paper]

Kuhn:

I didn't mean to be challenging the accuracy of what was said, nor was I indicating skepticism about the result but I really wanted to ask - I would say this is the sort of result appearing out of the mathematics that Ehrenfest, for example, who thought very often of adiabatic invariance as a key to the finding of a new quantum theory, I think would have jumped on. For him it would have been something that immediately indicates an unexpected characteristic of multiply periodic systems, that may tell us something about Nature, rather than just about the mathematics. I wondered whether you had any such sense yourself. That is, it is rather odd at least that you discover, doing this problem with a technique usable only for two dimensions that somehow or other the multiply periodic system acts two-dimensionally because of selection principles. Yet the selection principles come at this point not from the mathematics - I mean not in the sense that

later they come from group theory - but from a variety of rather elaborate physical arguments. That the method of this special characteristic of the mathematics suddenly fits to the selection principles to make the whole problem manageable as a two-dimensional problem, would have caused some people to be just delighted and to think, "I've got something really fundamental here ."

Dirac:

I don't remember the details of the work. Is the work correct?

Kuhn:

I have not been able to study any of these papers closely, but at least it makes good sense.

Dirac:

Yes.

Kuhn:

I think certain of the people who were doing this sort of work at the time, might have been disposed to try to make a great deal out of that.

Dirac:

Well, I didn't feel it was very important. I felt the whole of this work was really very feeble and that we hadn't got the right clue.

Kuhn:

But as far as you could remember this didn't seem to you to be very likely in the nature of a clue that could point to something deeper.

Dirac:

No, I would probably have gone on to work on it if it had..

Kuhn:

Did you have any of the feeling about adiabatic invariance that Ehrenfest I think in particular did have. I mean this was for him something to be pursued as a clue to 'quantum mechanics,' In fact, the term seems to originate around Ehrenfest. I first see it in Burgers' thesis. Born, I think, feels that he was the first to use it but that's much later and you find "Quanten Mechanik," in quotation marks, beginning to figure as early as 1918 around

Ehrenfest with the notion that adiabatic invariance somehow point to an underlying regularity in Nature which is closely associated with the heart of the new theory, whatever that will be.

Dirac:

Well, people did feel that it was a useful principle. That and the Correspondence Principle was all one had to work on in those days. But still it was feeble because it applied only to very special conditions.

Kuhn:

What about the Correspondence Principle? Did that seem something very fundamental or did it seem to be a crutch?

Dirac:

It always seemed to me to be a bit vague. It wasn't something which you could formulate by an equation.

Kuhn:

Yes, I suppose the answer is just that you couldn't write it as an equation. After you had written your equation you would know what the Correspondence

Principle said should happen to it, but you'd have to have the equation first.

Dirac:

All it said was there was some similarity between the equations of quantum theory and the equations of classical theory. I don't believe it was more definite than that.

Kuhn:

Well, it said that the Correspondence should be manifest as h goes to zero, and in some formulations that it should reduce to an identity, as h goes to zero. Of course, I think that in the earlier forms where it's applied initially only to frequencies but then also to frequencies and amplitudes, that you've said that the equations should become the same for large quantum numbers or for small h .

Dirac:

People held onto the Correspondence Principle and the adiabatic principle because it was the only thing they had to hold onto.

Kuhn:

Well, my feeling is that in Copenhagen in particular the attitude toward the Correspondence Principle would have been that it was something more than a heuristic tool in the sense that you're suggesting. Is that wrong? It may well be.

Dirac:

I'd say that what the Copenhagen people think is correct by definition, and probably I didn't fully appreciate the Correspondence Principle. I expect that's the answer • I didn't fully appreciate it because it didn't have the kind of precision which I like to have.

Kuhn:

Do you suspect that Fowler felt much the same way about it?

Dirac:

No, I think he was quite happy with it. The adiabatic principle was more definite to me than the Correspondence Principle because it did give some precise equations.

Kuhn:

I ask then just once more in this period before the Heisenberg paper appeared. Do you remember concretely other problems that you did which didn't come to publications I can give a hint on one and I don't know what it means, but there is a letter from Fowler to Bohr in Copenhagen in which Fowler among other things transmits a memorandum or something of the sort of yours, and so far as I know that no longer exists, which deals with a problem that he simply refers to as Wentzel's phase. I don't know what Wentzel's phase is. Undoubtedly if I read all the Wentzel papers from these years I might, but there's at least nothing in the titles

Dirac:

You'd better ask Wentzel about that.

Kuhn:

Well, indeed I shall ask him but he won't be able to tell me

Dirac:

Why shouldn't he be able to?

Kuhn:

He may be able to tell me what Wentzel's phase is, he won't be able to tell me why you were interested in it. That is, I raise this not to find out what Wentzel's phase was, which we can do any one of a number of ways, but to see whether it brings back any more detail to you about that problem or other problems you may have been working on that one can't get at now from the published literature.

Dirac:

Well, I would have to refresh my mind about what the Wentzel phase is. I might remember something. I know I worked quite a bit on the Weyl's electro dynamics and that's not connected with quantum theory at all.

Kuhn:

Did you think it might supply any hints toward the quantum problem?

Dirac:

No, I thought it might be a correct description of Nature and I was quite disappointed to find that it

wouldn't work. I made a detailed study of the field around an electron and I found that the equations would really require that the charge is not constant but varies with time by a certain coefficient depending on the mass, Or is it the other way around the mass isn't constant and varies with a coefficient depending on the charge. One or the other of the two possibilities.

Kuhn:

You didn't publish that?

Dirac:

No, I didn't publish it. But it meant that the Weyl theory is untenable, and everyone seemed to agree that the Weyl theory was untenable although for different reasons.

Kuhn:

Do you have recollections of any other things of this sort?

Dirac:

Well, I was working a great deal on action and angle variables. That was my main subject of work.

Kuhn:

What sort of things do you think you were trying to do with them?

Dirac:

I suppose the main problem was to try and introduce them or something corresponding to them, for systems which are not multiply periodic - the helium atom in particular. Trying to extend dynamical theory to have something to play the role of these variables for systems which are not multiply periodic. It seemed to me at that time that that was the only way in which one could develop quantum theory.

Kuhn:

If I might move now to your introduction of the distinction between c numbers and q -numbers in the first of the papers that does an application to the hydrogen atom. [Paper No. 9] There had been no hint of something of this sort in the first paper. [Paper No. 8] In formulating the two sorts of numbers one has to find one which has the algebraic properties of the quantum mechanical entities, the

other which can be represented by algebraic numbers, and then some way of correlating these two. This is a terribly big step and very cleanly formulated in the paper. How did you come upon the distinction?

Dirac:

Well, that was the whole difference between classical theory and quantum theory. You might ask why I was able to manipulate the symbols so easily. I think I had gotten used to symbolic methods earlier. The (stress diagrams) which engineers use is something like symbolic methods for getting results. And in projective geometry some of the methods tend to approach symbolic methods.

Kuhn:

I've never done any projective geometry and I hadn't realized that they were so often symbolically formulated.

Dirac:

Well, I think someone called Grassmann invented a symbolism for working out things in projective geometry.

Kuhn:

Grassmann?

Dirac:

I'm not quite sure that I've got the name right.

Kuhn:

In any case there was a book with which you worked at one point that at least made some reference to this sort of treatment.

Dirac:

Yes. Fraser's lectures were built very much on one to one correspondence deducing results simply by working from one to one correspondences. It's a very powerful, sort of a magic way of getting results. That sort of training was very suitable for later on developing symbolic methods.

Kuhn:

Does the one to one technique that you think of their bear some resemblance also to the way you develop an algebra for q-numbers?

Dirac:

No, not in detail.

Kuhn:

You speak in this paper of the correspondence between q-numbers having an analogy to c numbers and to classical observable quantity. You say there are occasions on which more than one q number will be available as the analogue for a single c-number and then you have to make a decision as to which is the more important, and give as an illustration of this the fact that you can develop for frequency in q number notation things that I would want to say are the analogue on the one hand to classical orbit frequency and on the other hand to classical transition frequency; I wouldn't want to say that there are two q-numbers which are equivalent, which are analyzed for the same c number, I've not quite understood really what that problem was for you, I mean what a more proper one to one correspondence would have been. I think there's clearly something that was bothering you.

Dirac:

Probably the idea was simply wrong. I don't remember this about the frequencies.

Kuhn:

This one I would be grateful if we could have a look at.

Dirac:

That's a question of naming the frequencies in the quantum theory.

Kuhn:

I take it that that phraseology indicates that you would really have been better satisfied, or felt that the situation was neater, if in practice there had been only one thing that showed up as a frequency that this is an aspect of ambiguity which has to be eliminated by intelligence that goes beyond any directions you are able to give.

Dirac:

Yes, I would agree with that. It's largely a question of terminology.

Kuhn:

But I'm perplexed as to why, in the light of the whole structure of this theory, it comes as a disappointment to have two things —

Dirac:

The correspondence between the classical and quantum theory is not quite so close as one previously expected.

Kuhn:

But isn't it exactly that this is because there is the q-number formulas are all modeled on this classical formulas. In the classical formulas you do get two sorts of frequencies, the orbital and the transition. Here again you get them again and it seems to be an exact, it seems to be an example of the analogy rather than a break-down of the analogy.

Dirac:

Well, it means a lack of one to one correspondence between quantum and classical theory.

Kuhn:

Classical in the sense of pre-quantum mechanical classical not in the sense of Bohr classical.

Dirac:

Yes, pre-quantum. Yes, it is classical in that sense, and it is a failure of one to one correspondence between quantum and classical theory.

Kuhn:

Classical in the sense of pre-quantum mechanical classical not in the sense of Bohr classical.

Dirac:

Yes, pre-quantum. Yes, it is classical in that sense, and it is a failure of one to one correspondence between the two theories.

Kuhn:

Yes, I guess this is what I was missing, that you shouldn't have had such things as both transition frequencies and orbital frequencies.

Dirac:

Classical here does mean before Bohr orbits.

Kuhn:

Before Bohr orbits you only had orbital frequencies and by the same token as you come around again you should only have one sort.

Dirac:

Yes.

Kuhn:

This brings me to your paper on the Compton effect and one of the problems I want to raise here is one that I think you may want to talk about and I'd certainly be very grateful, not only with respect to this problem but also with respect to a good deal that happens later, if it leads to that. In the first paper since before you have gotten into quantum mechanics, in the Heisenberg sense in which you attempt to relativism the equations, had you felt from the beginning that the fact that they were non-relativistic equations was a limitation?

Dirac:

Which paper are you talking about now'?

Kuhn:

This is the paper in which you introduce a four dimensional formula, in which you introduce time and energy as conjugate variables. (Paper No.11)

Dirac:

The one on the Compton scattering, yes. Well, of course, I knew from the beginning that the theory ought to be relativistic. Relativity was very well established by then.

Kuhn:

When you were working on action and angle variables, had you attempted to do that in a relativistic formulation? Had you had any notion that there might be then a clue?

Dirac:

I can't remember doing that. I don't think so, because the main problem there was the helium atom

and relativity doesn't come in when you've got a heavy nucleus

Kuhn:

What I'm in part here groping towards I think there were a good many rather different attitudes in the profession as to what sort of validity was left to a non-relativistic approach in view of the existence of relativity theory.

Dirac:

Well, it was certainly a very good approximation when you've got an atomic nucleus present. I think my engineering training was a help in reconciling that kind of an approximation which is so obviously sensible from the practical point of view,

Kuhn:

In the forward to the third edition of your *Mechanics* you speak of one very important change in this edition being the sense in which you've now adopted the non-relativistic state formulation.

Dirac:

Yes, I remember making that change.

Kuhn:

But you had thought at one time that the quantum mechanics should in its neatest and best form be a relativistic quantum mechanics and that there is some sense here that there are things that we cannot preserve in relativistic mechanics that seem very natural to quantum mechanics and that we only get in a non-relativistic form.

Dirac:

Well, when I wrote the first edition of the book, I tried to present the theory in terms of basic ideas which are relativistic and I was lecturing on the subject every year to students and I found that it just wasn't practical in a first presentation of the subject to people to whom it is quite new. You want to present the basic ideas in the simplest possible way and for that purpose it is more convenient to use non-relativistic concepts.

Kuhn:

But surely what you say in the preface to the third edition goes deeper than the question of pedagogic practicality. It seems to me that it winds up with the

remark that there is a problem that perhaps we all ought to take more seriously.

Dirac:

I don't remember just what I said. I think it still applies to the present day. People who write textbooks on field theory try to formulate everything in terms of relativistic concepts and I think that they make the theory more obscure than it need be by doing so.

Kuhn:

But do you think this is really a problem in obscurity of presentation, not of there being any sort of more fundamental miss-match between a relativistic formulation and a fully adequate quantum theory?

Dirac:

I think there is a miss-match and I think it's not yet solved.

Kuhn:

I take it that this preface does hint at that, or perhaps more strongly than hints.

Dirac:

Well, maybe it does. I've had the idea a long time that there is some basic discrepancy here. I didn't know it went back as far as that.

Kuhn:

What I'd like to do is to get you to talk about that, your early and later experiences in trying to keep quantum mechanics relativistic.

Dirac:

Well, I only know about much later experiences. I have some difficulty in remembering the beginnings of these ideas.

Kuhn:

Well at what point would you feel fairly sure of saying "I was really beginning to think this problem might be fundamental"?

Dirac:

Well, just during the last few years in some of my lectures I've been saying that I think the four-dimensional picture of the world is not the complete

answer. I don't know whether you are interested in these very late developments. It's something which has been growing on me for a very long time.

Kuhn:

To what extent does your reason for saying that the four-dimensional formulation is not the final word grow out of problems of the four-dimensional formulation by itself and to what extent does it grow out of the problem of matching it to quantum mechanics? That obviously doesn't cut a nice logical line between two sets of problems, but —.

Dirac:

I expect that it began through my finding out that it is difficult to teach students if you work with the four-dimensional picture all the time. That seems to suggest that the four-dimensional picture is not really so good as it might be. If it was really the final word this difficulty should not occur• And all the vacuum fluctuations that you get with field theory contribute to this point of view.

Kuhn:

But you think then that this sense of miss-match is really very likely recent history, that you were into the middle thirties, say, pretty well content that the problems that remained in matching quantum mechanics and relativity were problems of detail that would surrender to

Dirac:

Well, I thought that there were difficulties because of the infinities in the field theory. That occurred pretty early, I think already before 1930. It was a disappointment because the first few years one thought that the quantum theory of Heisenberg would be the answer to everything. Then as soon as one began to develop field theory, one saw that it wasn't.

Kuhn:

I had hoped that there might be roots of this problem of relating the two that went as far back as your earliest introduction of relativistic formulation. [Paper No. 11] It is the one in which you introduce W and t as conjugate variables in a Hamiltonian

formulation. But that's entirely new to me when I see it there but I'm not at all clear as to whether there had been other formulations of that sort before. I mean in the classical Hamiltonian equation with W and t occurring as conjugate variables.

Dirac:

I think it has occurred in classical theory but I'm not quite sure just where.

Kuhn:

You develop it classically first and then simply apply commutation relations to W and t ; the classical formulation is one I hadn't seen. I don't think it's in Whittaker, for example.

Dirac:

I think it is rather standard that you can count time as an extra variable and introduce something conjugate to it.

Kuhn:

Do you think it was relatively standard at the time? I don't know of another place where this point had

been put previously in this way, but I'm not at all sure it hadn't.

Dirac:

Well, I think I might answer you 'in much the same way that when I wrote that I felt that probably it had been done before, but it was less trouble to me to present it as something new than to search for a reference. A good deal of my work was like that • It happened rather often that there was something which I thought was very likely done before, but seemed to be a great nuisance to look through all the references to try to find it, and if it doesn't take much trouble to publish it, one could publish it again without claiming either that it's new or that it has been done before. For example the Hamiltonian theory for an electron moving in a given external field. I think that's probably been done before although I haven't found the reference to it. Just the classical equation, working with the Hamiltonian 'which is quadratic in delta as veil as quadratic in the three momentum variables.

Kuhn:

My guess is that it's also been done before, but I'm not quite certain.

Dirac:

It quite possibly has been done around 1910 or something like that. Maybe Cunningham could answer that. I suppose I was lazy with regard to looking up references.

Kuhn:

To what extent, if you knew you had seen a point of this sort somewhere else, but couldn't remember where, would you have tried to find it or have mentioned it as "someone has shown" or something of this sort. Because I take it when you mean that you didn't look it up, you meant that you didn't bother to go back and check all the literature to see whether somebody had in fact done this before. That so far as you knew, nobody had done it but -.

Dirac:

Well, I think with regard to using W and t as conjugate variables in the classical theory, I did feel

that I had seen it somewhere before but I couldn't remember where and it might even have been mentioned in a lecture. I think I had seen it before; it's not my own invention. It's probably in Whittaker or —.

Kuhn:

In this first Compton effect paper, you treat the field as a classical variable with no attempt at this point to quantize it.

Dirac:

That is so, yes.

Kuhn:

Did you think of that as being an approximation at that point? Would a fuller theory have introduced the field also as q-numbers? [Brief interruption]

Dirac:

I don't think I felt conscious of it as an approximation. So often in one's engineering work one makes approximations which one isn't conscious of; one treats a body as rigid although

actually it has elasticity in it and it gives; one just isn't conscious of these approximations.

Kuhn:

Well, Born and Jordan, and then again Heisenberg, Born and Jordan, had both done something treating the fields as quantum mechanical quantities as quantized. This is really after that.

Dirac:

Yes, after that, not at that stage.

Kuhn:

I wondered whether from the beginning you had taken it for granted that in a full treatment the magnetic and scalar potential would also come out as quantum mechanical quantities, would also satisfy some sort of commutation relations.

Dirac:

I can't remember whether I thought of that at the time I wrote the quantum paper. Probably not.

Kuhn:

Some people seemed to have the feeling that this whole attempt to quantize the field, at the very start, was nonsense and that there was nothing that had happened yet that made it necessary to break with electromagnetic theory in those.

Dirac:

Well, one had the treatment of the hydrogen atom where the field is certainly not quantized, it's just the Coulomb field. That worked very well. When I wrote that Compton paper I expect I felt the same way about the field in the work on the hydrogen atom.

Kuhn:

Do you remember reacting at all to those late sections of Born-Heisenberg-Jordan work in which they almost from the start, and I think this was largely Jordan's doing, pushed into treating the field in matrix terms also?

Dirac:

I think that was later, I don't remember just when it came out.

Kuhn:

There's field quantization, of a sort, in the immediate follow up on the Heisenberg paper.

Dirac:

Yes.

Kuhn:

So I think you'd have seen those papers by then. Let's come to the Schrödinger paper • Did you know of that work, the Schrödinger wave equation immediately?

Dirac:

People told me about it, but I think I was rather delayed somewhat in reading it just because I had a good method of my own • If you've got one good method and can devote your whole attention to it, you don't really want to be diverted onto another

method. So at first I did not pay attention to it, but I did later on because I was told it was so important.

Kuhn:

How did you feel about it when you saw it?

Dirac:

I suppose one does feel a bit annoyed if one has one method which works perfectly well and then one finds one has to learn about another method also, and fit the two together.

Kuhn:

Did you suppose from the start that they were presumably equivalent? Supposing that you did not think that they were equivalent at the start. Did this mean that presumably Schrödinger's was just wrong? Or that one or the other was wrong?

Dirac:

Probably my first reaction of all would be to think that Schrödinger was wrong, because it was so different from another method which one felt pretty sure was right. I think that was my first reaction.

Kuhn:

How about the manner in which he interpreted the equation —.

Dirac:

How did he interpret it in the first place?

Kuhn:

Well, that is a real wave. He supposes, for example, that radiation is perhaps due to interference effects between waves of different frequencies. The frequencies of these waves are the orbital frequencies and there will therefore be an interference effect that's the difference between two orbital frequencies.

Dirac:

The question as to whether the waves are real or not would not be a question which would bother me because I would think upon that as metaphysics.

Kuhn:

He would also think that the wave packet which one decides ultimately tells you probability density is in fact the electron.

Dirac:

The wave packet didn't come in until quite a bit later. I don't think that it did.

Kuhn:

Quite early Schrödinger is using something rather like a wave packet - certainly in the problem of spreading. He talks about dimensions of the electron, conceived as the spreading of the wave function, as compared with atomic dimensions, very nearly at the beginning of that work.

Dirac:

Well, for people who want to interpret the waves as real, the spreading of the wave packet does present a serious difficulty.

Kuhn:

But this is something which very early in the debate with Heisenberg, for instance, gets pointed out — this spreading problem, so that in this sense, while the problem may be of a metaphysical type there are non-metaphysical issues that it presents that are picked up pretty early by some people.

Dirac:

I did not concern myself with this question of the wave packets at that time.

Kuhn:

You know Heisenberg and Born in particular both jumped very hard, and in Born's case sometimes quite intemperately on the sort of thing that Schrödinger is saying about the wave functions whether the wave function itself is or is not — I'm not sure to what extent this happened before the recognition of the identity of the two. Now I wondered whether that sort of issue had involved you at all at the start or whether you had reacted —.

Dirac:

No, it didn't. I was quite happy just to go my own way and let the other people go their way with their theories.

Kuhn:

In a number of places one finds that to people who had paid very little attention to matrix mechanics suddenly quantum mechanics begins to seem all right with the wave equation. Was there any of that also at Cambridge?

Dirac:

I don't remember. It might possibly have applied to Darwin, but I'm not sure about that. It's a great pity that Darwin died just a short time ago. But Fowler was quite happy with the matrix mechanics.

Kuhn:

You wouldn't say that with the coming of wave mechanics more people at Cambridge began to get really seriously involved than had been on their way to doing this before.

Dirac:

I think the experimental people were a good deal happier with the waves • That was something they could understand.

Kuhn:

When you began to think that perhaps the q-number approach and the wave function approach were equivalent, did you attempt yourself to find an equivalence between them?

Dirac:

Other people did it first. I heard that other people had established an equivalence between matrix mechanics and wave mechanics and I just read their work.

Kuhn:

You hadn't tried it yourself?

Dirac:

I hadn't tried it, no.

Kuhn:

By the time you heard that they had done it, do you suppose you were pretty well prepared for that or had the idea really —?

Dirac:

I think I was prepared for it when it came.

Kuhn:

You yourself first produced the Schrödinger equation in the paper on the theory of quantum mechanics. [Paper 14) You generalize it by producing a time dependent form which is linear and also by giving, as you produce it, a number of hints of things that are going to show up as transformation theory a little bit later. You talk about j —.

Dirac:

The linear equation that you are referring to is not the equation of the electron is it?

Kuhn:

No, no, no. Again, the timing is hard to tell because the two papers are too nearly at the same time.

Schrödinger's early equations, when they've been in a time-dependent form, have involved the second partial with respect to time. He corrects this sometime around mid-summer of '26, which is around the same time that your paper on the theory of quantum mechanics goes in. You both get at this point a form in which only the first derivative with respect to time occurs. These are quite independent? Is that right?

Dirac:

I expect what happened was that after people had established the equivalence between the matrix and the wave theories, I just studied their work and worked on it and tried to improve it in a way that I had done several times previously. I think the transformation theory came out from that.

Kuhn:

The transformation theory doesn't in any very clear-cut form exist in the paper I mean to be pointing to at the moment. That's a slightly later paper. This is a paper that you submit in August, 1926, called "On the Theory of Quantum Mechanics," [Paper No. 14] and the paper that clearly does have transformation

theory is one that goes in in December called the
 “Physical Interpretation of the Quantum Dynamics.”
 [Paper No. 16)

Dirac:

Well, I better check up on this one on the theory.

Kuhn:

This is the one that’s now perhaps best-known
 because it’s got the Fermi-Dirac statistics. In fact,
 it’s got an awful lot of things, not obviously parts of
 the same problem although

Dirac:

By the way, do you know this story that I wrote
 about Schrödinger?

Kuhn:

No, I don’t know this story, and I certainly should.

Dirac:

About the discovery —

Kuhn:

I have read the obituary that you did of Schrödinger.

Dirac:

Well, this story here, I've published it before.

Kuhn:

I know why I don't know this, my copies of the Scientific American the May American had not arrived when I left Copenhagen. [Looking at the article) Yes, this is terribly interesting; you also tell it in the obituary, which you pointed out to us before, so I did know this.

Dirac:

The editor asked me for more details about this and I tried to reconstruct the equation. This is what I imagined the equation to be but I presume that his first equation was this one. He didn't actually write it down to me but I think it must have been that, because it was a generalization of de Broglie's equation and a relativistic one.

Kuhn:

Excellent. I wondered a good deal myself what it would have looked like. This is it, yes. You immediately go to this form in which minus W is minus in partial with respect to t which is not Schrödinger's equation at that point. Actually this is not for you a great big step because it fits so exactly with the W, t formulation that you've given in the Compton effect paper • Without that background it would have been at least a much larger —.

Dirac:

I presumed that Schrödinger had this right from the beginning

Kuhn:

No, no, no.

Dirac:

Well, if he had this equation, then he had it.

Kuhn:

Ah, Ok. But then let me only suggest that that may be an argument against his having had this equation,

because there's no question that in the early non-relativistic papers up until part three, until the dritte Mitteilung, when he writes down a time dependent equations at all, he writes it as a wave equation with a second partial, with respect to t and this would have to give him a in it.

Dirac:

Well, you mean he would have this equation without putting d/dt equal to W .

Kuhn:

Yes.

Dirac:

Yes, I see, all right.

Kuhn:

In the dritte Mitteilung, which is actually the fourth paper in the series, he then does correct this time dependent form and comes out more or less at the same time you do with the form that's linear in time. But then you go on here in this to talk about the solutions of the linear equation $FP = 0$ [reading): "the matrix representation we have obtained is not

unique since any set of independent eigen functions will do.” Now here, as soon as you start to talk about the characteristics of different sets and then producing one set that will diagonalize W one’s taken an important step toward transformation theory although clearly the thing stops right there in this paper. There’s certainly nothing like the ‘j function as a transformation function. This leaves me wanting to ask whether this brings back any of the ways in which this set of ideas fits in with the development of the fuller transformation theory. I’m terribly interested to know how you were driven toward the fuller transformation theory. I think that paper is of almost all of them to me the most exciting.

Dirac:

The transformation theory one?

Kuhn:

This is not meant to be a historian’s evaluation of your work, but that paper I find in particular extraordinarily exciting to read • But there are elements of it here.

Dirac:

I can't remember how I came to think of it. I know it was a question of working on it for a good long time and gradually changing from one form to a slightly better form. I didn't think of it in the first place in the form in which it was published.

Kuhn:

Bohr, in a paper that he did for the Heisenberg Volume, makes the remark that you had said when you heard Heisenberg's report on colloquium in Copenhagen on a paper on fluctuations phenomena that you suddenly saw it was a transformation theory or something of this sort. This story doesn't quite make sense to me, but it does suggest that there may have been something quite important about this colloquium report of Heisenberg's in the development of your transformation theory paper. [Paper No. 16) Do you remember that? It was the report in which Heisenberg points out that the time averages of energy, energy squared, and so forth in a resonant system come out in such a way that one must have a discontinuous passage from one energy level to the other, that one can't have a traceable

oscillation in time. You yourself, in the transformation theory paper, very early mention this still unpublished work.

Dirac:

I think that Colloquium report came while I was working on the subject.

Kuhn:

Yes, the date would come after this paper, but before the —

Dirac:

I think I did appreciate it that it fitted in with my work.

Kuhn:

But you don't remember having had a sudden flash of illumination in the course of contemplating this result?

Dirac:

No, no. I think I just felt that it did fit in a way that things ought to fit.

Kuhn:

Yes. This was your own reference to the —.

Dirac:

Yes. I think the answer to that is that I did not get a sudden flash at that time. I don't think I could get a sudden flash from a conversation. I could only get it by turning things over and over.

Kuhn:

Well, if I remember Bohr's words in that paper they'd not be incompatible with your having gone home with the idea and turning it over and over and over and then seeing something in it.

Dirac:

I think I'd already gone a good way towards that idea when I heard Heisenberg talk on it.

Kuhn:

Coming back to this one. You go immediately -.

Dirac:

Do you know the date of Heisenberg's talk?

Kuhn:

Not exactly. It would have been in that fall, but whether early or late in the fall I've got no notion. You go immediately from the generalization of the Schrödinger equation to using it on the problem of similar particles. It works very nicely; on the other hand, just as a line of development in the paper this seems like quite another problem. I wondered how the two had gotten together. You know the problem you solve here is just the problem that Heisenberg at substantially the same time solves, officially using matrix mechanics. It isn't totally clear to me that he didn't get some of that by utilizing wave equations and then reformulating it in a matrix fashion. I wondered how this problem itself developed but wondered particularly about the thing that emerges here that this is the point where you feel you have to throw away the formulation in uniformizing variables which had been the standard q -number interpretation up to this point. [See paper 14: "On the Theory of Quantum Mechanics," Proc. Roy. Soc. A 112 (1926) paragraph 3)

Dirac:

Well, this piece of work was separate from the other one and the only reason why I didn't put it in a separate paper was that I thought the papers would be too short. I was changing over to the wave function formalism because it was more powerful than just the q-numbers.

Kuhn:

When you speak of this as separate do you have any notion of the order, for example, in which they came? To reach this conclusion about the inadequacy of the uniformizing variables you don't need to have the Schrodinger equation. And in a sense your whole experience trying to use action and angle variables for multi-electron problems may have led you to suppose that they wouldn't work for a two electron problem with q-numbers either.

Dirac:

I think they would work apart from the symmetry questions. Wait a minute, they work only for multiply periodic system, don't they?

Kuhn:

I think this is the only way in which they have been developed.

Dirac:

Oh, yes, I should have said that. That's wrong, they work only for multiply periodic systems so they won't work in any case for helium.

Kuhn:

This is exactly the sort of question I mean to be asking. You produce an argument based on symmetry to show that the approach through uniformizing variables breaks down. Now in a sense —.

Dirac:

Did I actually say that?

Kuhn:

Yes, I think so. I think the first place you say it is, [paper 14, p.668) "It may be shown, however, that there is no set of uniformizing variables for a system containing more than one electron." Now, there's

one sense in which you must have known that from the start because you'd had that trouble with the old Hamilton-Jacobi techniques. But there's another sense in which you seem to have been expecting it to work in fact surely you had because there's old paper on the nodes in quantum mechanics [Paper No. 10) which deals with multi-electron systems and uses uniformizing variables— and I wondered at what point this conclusion came in.

Dirac:

I better check out this reference here where I said -. Because this second sentence seems to be a doubtful one. I wouldn't expect that —. [searching for paper] ... [examining paper) I've forgotten the exact sense in which I'm using uniformizing variables.

Kuhn:

Well, I have been reading them as meaning the same thing as action and angle variables, that is, as a set of variables such that at least classically the I think they are indistinguishable from a set of action-angle variables. The H is a function only of the momentum variables. The q 's, the original q 's and p 's are

multiply periodic. [see, for example] paper No. 9, paragraph 4.)

Dirac:

Well, in that case it would not work for a two electron problem even if the electrons were not equivalent and symmetry conditions don't arise.

Kuhn:

Well and good, but there is the paper [Number 10] that immediately follows this one in which I think the same approach is utilized and one proceeds ahead to multi-electron problems. Now I May be wrong.

Dirac:

This is treating it only by an approximation which does allow uniformizing — action and angle variables. I think it's essentially a one-electron problem with a core like one worked on in the Bohr-Sommerfeld quantization. People worked on the spectra of the alkali metals and things like that and they used action and. angle variables but they were just in an approximate model.

Kuhn:

No, I do see that and I hadn't seen the sense in which this approach would, of course, justify it.

Dirac:

This paper [Number 10) on the nodes involves the same approximation as the old work of Bohr-Sommerfeld.

Kuhn:

I'd realized all along that it did, but I had not quite seen the relation of that to the question of the legitimacy of the uniformizing variables in the multi-electron problem, that they came back in with this approximation. This still leaves me with trying to see whether we can find out any more about the source and sense of this announcement that uniformizing variables won't work with more than one electron. This makes this whole point somewhat stranger because in one sense one wants to say that you knew all along, that they wouldn't work because it's no longer a multiply periodic problem, but on the other hand pretty clearly this proof makes no reference to that; it's based on symmetry properties

and it does sound somehow or other as though this is a new idea to you and somewhat discouraging in terms of what it has to say about the previous approach. Or how fundamental the previous approach is.

Dirac:

Of course, at that time one didn't know how far the matrix mechanics would go towards solving problems like the helium spectrum. It could be that one supposed that one would have action and angle variables occurring even there because the whole thing was based on just a limited number of frequencies occurring, corresponding to the different matrix transitions.

Kuhn:

I would think that that was very likely the way one had felt about it, simply because there is no reference to an expectation that this whole thing's going to break down as soon as we get to two-electron problems, and because the proof that it breaks down, when it comes, is of such a different nature. It's a proof based on the symmetry argument.

Do you know how you came to trying the symmetry argument and to realizing it's consequences?

Dirac:

I don't think there was any difficulty about coming to the symmetry argument as soon as one faces up to the question that the two-electrons are equivalent. In fact, when two people do a piece of work simultaneously there's really no difficulty about the approach to this work.

Kuhn:

It at least indicates that somehow or other it was in the air but it doesn't mean that the two individuals came to it the same way.

Dirac:

Well, it means that it was pretty obvious. Things which are not pretty obvious are those which take many years before there's a breakthrough. I don't think it's any puzzle how someone gets the idea which leads to the break.

Kuhn:

Yet I refuse to take it that because two people got it, it must have been easy. I will apply this in most cases but when the two people are yourself and Heisenberg

Dirac:

I think it was easy and that any good research student would have got onto it pretty soon.

Kuhn:

Well, if anybody had said to him, “Figure out what the consequences of the indistinguishability of the electrons are.” But what was it like to pick up that idea?

Dirac:

I think people knew for a long time that the electrons were indistinguishable one from another.

Kuhn:

Yes, but I don't think they'd been using it this way.

Dirac:

Of course, as long as you have electrons moving continuously, it doesn't bring in any new situations. It's only when you have jumps that you have new situations coming in. I suppose the only point to appreciate is that this bringing in of jumps does bring in some new and quite indistinguishable particles.) It's a sort of like the negative energy states: it's only when the jumps come in that they become important.

Kuhn:

But you've got no recollection of how the notion of asking that set of questions came about.

Dirac:

No, no. In any case it was pretty straightforward.

Kuhn:

With the case of the problem of transitions to negative energy levels it's pretty clear that if you got the equation with the negative energy levels you'd have to ask yourself, "Can I avoid transitions?" and then answer, "No." There's nothing here that I'm

aware of that makes it pressing for you to ask the question you're here asking.

Dirac:

Well, I think there is. If you just talk about transitions, 'why do two electrons change places?' And try to fit such transitions into the theory. You are pressed in that way.

Kuhn:

Yes. Was it hard to drop the idea of the uniformizing variables as a general approach?

Dirac:

Not when one has something else to replace it, namely the Schrödinger wave functions. One doesn't mind dropping an idea if one has another powerful one to use in its place.

Kuhn:

Then you go on right from this to the statistics.

Dirac:

Yes. Again, because it simply made too short a paper if it were put separately.

Kuhn:

But this does fit with the statistics pretty well.

Dirac:

Yes, yes.

Kuhn:

Because it leads you immediately to the symmetric and anti-symmetric —. Had you been perplexed about a reason for the exclusion principle before you got to this? Was that a problem that had—?

Dirac:

It hadn't worried me. If a theory is advancing in one place, one isn't bothered by a temporary stagnation in another place.

Kuhn:

Of course, the exclusion principle exists even before the theory.

Dirac:

Yes. Before the beginning of quantum theory.

Kuhn:

Yes. It was rather the same thing you say about spin. “But why should it be that way” could be, and I think it was, asked in some places about the exclusion principle. “Why shouldn’t two electrons .j Now here it suddenly drops out but that had not been a source of vexation for you — the apparently ad hoc nature of the exclusion principle.

Dirac:

No, there were so many things one didn’t understand. One of the main things was the difference between positive and negative electricity which seemed to be something fundamental in Nature.

Kuhn:

People did not think it was enough to say, “Look, there are two types of particles and one of them’ s large and positive and one of them’ s small and negative.”

Dirac:

Well, people did accept that but it did mean there was a difference between positive and negative electricity. There was not symmetry between them although the electromagnetic equations were symmetrical.

Kuhn:

Who worried about that? Do you remember?

Dirac:

I don't think "worried" would be the right word to use, One just accepted it as a principle of Nature.

Kuhn:

Well, there's a terribly big difference in attitude between accepting something as a principle of Nature and feeling that, "All right, yes, it's a principle of Nature but darn it, everything else I know about Nature makes this an uncomfortable principle because I expect symmetry; there ought to be more to be said."

Dirac:

Well, there was so little known about atomic theory in those days.

Kuhn:

But when you start out by saying to me that there is another question like the question of why the exclusion principle was, namely, “Why this asymmetry?” that does somehow or other mean that although people may have been sure that the asymmetry was there, they were bothered by it, that it seemed unnatural or that it seemed to call for more explanation.

Dirac:

I don’t believe that’s so. I think people accepted it as the natural order of things and that’s why it was so hard to think of the positron.

Kuhn:

My impression was that people simply had accepted it and hadn’t asked, “Why the asymmetry?” but I think people did ask, “Why the exclusion principle?”

Dirac:

Yes, I don't remember asking it myself. I accepted it in the same way as the asymmetry.

Kuhn:

No, but you did ask, "Why spin?"

Dirac:

That was something which was much more recent and not so well-established and got thrown at one as a new idea.

Kuhn:

Is there any truth to the rumor that you made a bet with Heisenberg on the subject that you would find a more fundamental explanation than spin?

Dirac:

I think it's unlikely. I don't think I would make a bet on the future development of science because it is so uncertain. Who said that? Did Heisenberg say that?

Kuhn:

No, I don't think he did. I've heard it twice, both times I think with appropriate qualifications of the possible fallibility of memory and they are also not necessarily independent. One of them was from Oskar Klein, the other one from somebody else in Copenhagen, I think probably not from Bohr, but probably from somebody Bohr had told it to.

Dirac:

I might have said that I was dissatisfied with the theory and that I believed that someday a better theory would be discovered.

Kuhn:

No, we have a version which I will not vouch for at all which says that you were working on it and bet that you would have the answer within n months where n is some not overwhelmingly large number.

Dirac:

I don't think I've ever had that much confidence when I'm working on a problem. But I might have said that someday someone would find the answer.

Just like now I'll say that someday someone will find the reason for the constant.

Kuhn:

You feel that that has not already been answered.

Dirac:

No. I don't think anyone claims to have answered it. Except perhaps Eddington.

Kuhn:

Well, it was Eddington that I had in mind in asking.

Dirac:

We're really now in Copenhagen and I should ask you why you went in the first place but I gather that for a student of in quantum mechanics, wanting to go to Copenhagen was pretty natural. Were there any problems about arranging it? D Fowler was keen that I should go to Copenhagen and I think he wanted me to spend the year there. I was a bit worried about going to a country where I didn't know the language and. I was rather more desirous to go to Göttingen for that reason, because I did know a little German. We made a sort of

compromise between those two by going half a year to Copenhagen and half a year to Göttingen. I had no financial problems then because I had been awarded this studentship from the exhibition of 1851. Did I tell you about that?

Kuhn:

You came to St. Johns originally on an exhibition scholarship.

Dirac:

With an exhibition from the college and with a government grant. But that involved only a small amount of money and I was short of money. Two years later I was awarded another exhibition on a foundation which comes from the profits of the international exhibition that was founded in 1851. Then I had much more money and I had no trouble about financing the traveling.

Kuhn:

Did Copenhagen seem very different as a place to do physics. Different from Cambridge?

Dirac:

I had quite a different life there.

Kuhn:

In what way?

Dirac:

I was doing most of my work there in the institute instead of in my own room or in the library. I think I met people more often there.

Kuhn:

Did you work more with people or was the actual work still done very much by yourself?

Dirac:

It was done by myself. I've never been good at collaborating.

Kuhn:

Many of the people who went to Copenhagen from other places speak of the difference in the approach there or contrasts with the places they had been before. It was different from Göttingen, it was

different from Munich; these are the contrasts I hear most of.

Dirac:

Well, there was the personality of Bohr. That was the big thing there. Without Bohr I think there would have been nothing. I was very much impressed by hearing Bohr talk.

Kuhn:

What particularly impressed you about that?

Dirac:

Well, he's a deep thinker and he does think about all problems. -

Kuhn:

You said before that you wouldn't be concerned with problems which are meta physical; did you ever feel that way about any of the problems that Bohr presented?

Dirac:

No, he did not concern himself with metaphysical problems. But he concerned himself with problems

which are not related to science at all. For instance, when two gunmen each draw a pistol and each point it at the other, each one wants to kill the other one, but no one dares to shoot. What's the explanation for that? Why doesn't one of them shoot? Well, Bohr worked that out. You know that story? Well, it's a psychological question and if you make up your mind to shoot and then shoot, that's a slower process than if you shoot in response to some external stimulus. If you make up your mind to shoot, the other man will see that you've made up your mind to shoot before you've shot and he will shoot you first. Bohr bought some toy pistols and tried this out with various people in the lab.

Kuhn:

This is a problem presented by his having gone to a western?

Dirac:

I don't know how he thought of it in the first place, but they say that the gunmen know this. Neither of them dares to shoot because he will be dead before he can carry out his resolution if he does make a resolution to shoot.

Kuhn:

That's fascinating and it is a Bohr story I have certainly not heard before.

Dirac:

It is rather a slow process to make up your mind to do something and then to act on what you've made up your mind to do. It takes perhaps half a second or something like that while it's a much smaller fraction of a second to respond to an external stimulus. Another thing he thought out was about the stock exchange. Someone who studies what stocks to buy and sell and just buys when he thinks he's got something good to buy and sells when he thinks it's going to go down, such a person will do worse than someone who buys and sells completely at random. Bohr worked that out also. And do you know the reason for that? Did you hear that story?

Kuhn:

No, I haven't heard this story. Tell me the reason for it.

Dirac:

The reason is that there are some people with inside information, the directors of the companies and their friends and they are able to make a profit from their inside information; they may see that the company's doing badly. So they sell their shares promptly or they may see that the company's going to do well from confidential information they've received, so they buy the shares. They're able to make a profit. That's obvious. Now if someone buys and sells completely at random, on the whole his profits and losses will cancel each other, assuming there's no fraud, and that he deals with standard shares. The person with inside information cannot get money from the person who buys and sells at random. How can he get money? Well, he gets money from the person who just studies the situation and has a little information. For instance, the director who knows that the company is doing badly and proceeds to sell his shares, in order to get rid of his shares he has to sell them somewhat below their apparent value. The person who just studies the market without inside information sees some shares going for a lower price than their apparent value says, "Well, now here's

something good to buy.” And he’s the one who buys it and loses.

Kuhn:

This one I find unconvincing but I will have to work on it. ...

Dirac:

If too many people knew this Bohr theory and acted on it then the people with inside information would have to work the other way. It’s the sort of thought be liked to work with, and he impressed me very much with the extent of his activities.

Kuhn:

What was he working on in quantum mechanics while you were there?

Dirac:

I can’t remember what Bohr himself was working on.

Kuhn:

Did you talk much to him about your own work as it went on?

Dirac:

I think mostly Bohr was talking and I was listening. That rather suits me because I'm not very fond of talking.

Kuhn:

Did you give a colloquium on the transformation theory?

Dirac:

I'm pretty sure I did, yes. I did give colloquia from time to time. I can't remember in detail which ones were given. I don't suppose anyone has kept a record.

Kuhn:

I don't think there's anything like a book on them there. There are remarks about some of them from time to time, but I think there's no running record of the colloquia. Do you remember any particular colloquia you went to there?

Dirac:

I can remember Ehrenfest being present sometimes and what a useful person he was to have at a colloquium.

Kuhn:

That was your first contact with him?

Dirac:

Yes.

Kuhn:

He was just as useful in this period perhaps as he had been before?

Dirac:

Oh, yes.

Kuhn:

Because I think he himself felt that he'd never gotten the bang of matrix and wave mechanics.

Dirac:

He was very useful in getting things cleared up in the colloquia where things were not properly presented by the speaker. Ehrenfest was the most useful man that one has ever had at colloquia, I should say.

Kuhn:

Did he do this to Bohr also?

Dirac:

I don't think he did. I can't remember any example. I'm not sure.

Kuhn:

In some way they impress me as having been a good deal alike; but I think not in this way.

Dirac:

Yes, there was some symmetry between them. I suppose you know the remark that Bohr made so often, "Nicht um zu kritisieren, nur um zu lernen;" when cross questioning someone about the work,

and not wishing to offend them, he would put in that remark.

Kuhn:

Did you see much of Heisenberg there?

Dirac:

I don't think he was there very much during the period I was there. But one can look that up.

Kuhn:

I would have thought that he was there that entire fall except perhaps for brief trips out.

Dirac:

I don't remember.

Kuhn:

Did you see much of Klein?

Dirac:

Yes, yes, I think so. But I was in Copenhagen more than once and I may be confusing the two times.

Gamow was there one time. I'm not sure whether he was there at that time,

Kuhn:

I don't think he was there yet then. This was the period when the interpretation problem was beginning to loom quite large.

Dirac:

Yes.

Kuhn:

Certainly for you, certainly for Heisenberg, and for Bohr. Do you remember at all what was going on with respect to that problem?

Dirac:

I was mainly absorbed in developing my own ideas. I was content to carry on that way and have other people develop their ideas.

Kuhn:

I realize I've kept you at this longer than I should and it seems to me an excellent place to stop for today.

Interview Session – 5

Kuhn:

This would have been at Bristol still, the suggestion to look at quaternion's.

Dirac:

Yes, that was at Bristol, I think one of my teachers recommended me to it and said I'd be interested in it but I can't remember which one it was,

Kuhn:

One thing that particularly struck me in your article in the Scientific American in describing the various schemes that occurred to you as possible approaches to remaining problems, you constantly use pictorial models, I wondered to what extent with earlier papers you'd also had pictorial models in mind in advance and had done the mathematics to fit them,

Dirac:

Well, what example are you thinking of anyway?

Kuhn:

You mean what example in the article?

Dirac:

No, in the earlier —.

Kuhn:

Well, I would say it's a little bit hard thinking over the earlier things to see how you could have had pictures and the pictures certainly don't show at least in any very explicit form in the final papers, so I wondered whether this represented a later stage or the fact that perhaps these ideas are amenable to pictures as the earlier ones were not,

Dirac:

I think it represents the kind of work which I do when I'm not just stimulated by a paper and trying to improve a paper.

Kuhn:

Well, take the case of the electron wave equation. Now here you were not simply stimulated by a paper

and trying to improve it, though you clearly had a problem —

Dirac:

I was trying to improve the Schrödinger equation.

Kuhn:

I take it that the quantization of the Faraday field, the single lines of force, is actually one that goes quite far back. This seems to go with your paper on quantized-singularities in the electromagnetic field,

Dirac:

I didn't have it so far back as that,

Kuhn:

You didn't, But you do talk there in that paper about lines connecting magnetic poles the possibility of complete lines off to infinity that join or broken lines that have charge at the end, It seems to me a somewhat similar -.

Dirac:

I didn't have the same idea then that all the Faraday lines of force should be discrete I just mentioned in

lectures from time to time that I don't see how to develop them mathematically without getting into the usual kind of infinities. [Short pause to adjust the microphone]

Kuhn:

You think in the case of the older papers, the ones that we've particularly talked about so far that they all take off from an existing set of papers.

Dirac:

I think they rather do and that I tried to form a picture of that paper and getting a clearer picture of that paper enables me to write my own paper.

Kuhn:

Take the Schrödinger equation or the Klein-Gordon relativistic version of it: what sort of picture would you work with for that in trying to improve it? What sort of a picture would be the intermediary between the Schrödinger equation and your own relativistic equation?

Dirac:

I guess I'd be thinking of wave functions and having them multiplied up, forming some kind of density which one would picture spread about in space.

Kuhn:

In that sense, your own wave equation doesn't much modify the picture, does it? It certainly modifies the equation and the mathematics.

Dirac:

Well, bringing in the four components — that is something which one is really led to from the mathematics, and that's not something which is suggested from the picture.

Kuhn:

So in this case the mathematics really modifies the picture, rather than the picture suggesting —.

Dirac:

Yes. : Well, they will do both things, influence one another. So I am all the time trying to get better pictures. The renormalization theory I'm not very

happy about because of the absence of pictures. One can set up quite good pictures for the renormalization of mass. You would have the Wentzel field or a lambda limiting process. But I haven't found that corresponding picture for the renormalization of charge.

Kuhn:

When you speak of “picture”, you mean something that interacts with the mathematics, but that is not simply the mathematics. You're not simply seeing the symbols with a life of their own.

Dirac:

Someway which enables you to understand the equations independently of the approximate method of solving the equations. One ought to have well-defined equations before one starts to apply a perturbation method to solve them, and that is what one doesn't have with renormalization theory. But you can have it with respect to renormalization 'of charge if you use some such idea as the Wentzel field.

Kuhn:

The renormalization of mass?

Dirac:

Yes, yes, the renormalization of mass.

Kuhn:

You don't though in particular think of pictures which represented early attempts on one or another of the —. For example, did anything like picturing go with q-numbers?

Dirac:

I suppose I pictured the q-numbers as some kind of mysterious numbers which represented physical things. It was quite a good time before I appreciated that they were merely operators in Hilbert space. When I did appreciate that I dropped the terminology q-numbers. Q-numbers was something which was imperfectly understood, physical variables satisfying non-commutative algebra.

Kuhn:

When you thought of them as physical things, do you think that thinking of them in that way provided you guidance in their manipulation over and above that given by their formal mathematical property and their direct classical analogues?

Dirac:

I think it does to some extent. When an infinity turns up in the equations one can represent it by a finite quantity which approximates to it, I might often imagine that a series is cut off somewhere at distances which are much smaller than anything which is physically important. That would be quite permissible. That is one way of setting up a picture which will get rid of the infinities. Then one can try to figure out whether such a picture is relativistic or not. One can do that much better than if one worked with the mathematical infinities.

Kuhn:

All of these examples that you give are relatively late ones in terms of the period that we've been mostly concerned with so far.

Dirac:

Yes. Well, they're more fresh in my mind. The delta function came in just from picturing the infinities as something which approximates to them.

Kuhn:

Do you know when they came in? You first introduce them explicitly in the transformation theory paper but there are hints of them in two earlier papers.

Dirac:

I think I published them soon after I had the idea clearly.

Kuhn:

In the paper in which you first develop the Schrödinger theory, which we talked about last time, there is a rather cryptic footnote to the —. You developed a wave function as an expansion of the form CY in a sum or an integral, and then there's a footnote which says that there are some cases in which you cannot represent the new wave function as but as the limit of a series of such functions and

then in the very next paper which is the one on the Compton. effect treated by wave mechanics you use a delta prime function though you don't call it that and then the delta function as such then comes out very clearly and explicitly in the transformation theory paper.

Dirac:

Well, those earlier papers were really showing the need for it, I noticed the need for it very much in a certain place in Eddington's book. ..

Kuhn:

How early did you really see the need —? I guess I should ask which book -.

Dirac:

I could find it in a moment.

Kuhn:

I'd be very glad. The book then is Eddington's Mathematical Theory of Relativity, 1923 edition, and the footnote is at the bottom of page 190. This would be something you had noticed then really

before the delta function began to be directly involved in your own work.

Dirac:

Yes, yes.

Kuhn:

That's very interesting. It's just the sort of hint that I'm particularly glad to pick up. You refer in the transformation theory paper, very early in it, to a paper of Lanczos, published in 1926. I think it's written in 1925 and published in 1926. You say the transformation theory can in a sense, be regarded as a generalization of Lanczos' theory. Lanczos' theory is the paper which reformulates matrix mechanics in terms of integral equations and Kernels and then has a continuous matrix. Was that paper an important one to you? Was that one of the ones you studied quite carefully?

Dirac:

I don't think it was, no, as far I remember.

Kuhn:

He also comes within an ace of introducing the delta function in that paper. That is, he needs a form, a kernel which will serve as a unit matrix.

Dirac:

Well, I wonder why people didn't introduce it because it's just a question of notation.

Kuhn:

Well, I think many people must have been uncomfortable with the idea of a function that was that badly behaved. \therefore

Dirac:

The mathematicians would be.

Kuhn:

The physicists' functions were generally not even better behaved often than the mathematicians' functions, although the physicists apparently didn't worry too much about it.

Dirac:

Well, the physicists could understand it at once as a function which is narrower than any distance which he is ever concerned with, and the physicist doesn't need to go to the limit, he can just retain it as a function like that. I would always picture it in that way.

Kuhn:

So you have a picture with the sort of physical justification of it that it's an approximation function; all I need to do is make it narrow enough so that the distances involved are small compared with any I'm concerned with and then let it be a finite distance..

Dirac:

Yes, I would picture it in that way. I would do all my work with such a picture. So I have never worked with Schwartz's distributions because I find this picture quite adequate for all physical purposes.

Kuhn:

Reading your Scientific American article reminded me that when I talked with Heisenberg, one of the

things he said was that he remembered that at one time he'd had a conversation with you in which emerged a difference of opinion on strategy, You believed that one must handle one problem at a time and he felt that this whole group of problems had to be handled together or not at all.

Dirac:

Yes, yes, I know. A good many people hold Heisenberg's view, but it's just too difficult to solve a whole lot of problems together. I think several new ideas will be needed to get out of the difficulties in present-day physics and that these ideas will come one at a time with intervals of several years between them.

Kuhn:

There are two senses in which one can take this. One of them would be that one must in fact be working on a problem and not trying to worry about everything at once if one's going to make a contribution, that would be in the narrower sense. There would be the broader sense in which one means that we will in fact solve one of these

problems by itself without simultaneously solving a number of the others.

Dirac:

I don't think one should set out to solve one of these problems. One may be working on one of them and get something which contributes to a different problem. So I hardly think it's right to set out to solve one particular problem. And I'm just making a prediction of how it will go in the future. I think there will be several ideas needed and that they will come at intervals of several years.

Kuhn:

Is this sort of disagreement that's represented in your discussion with Heisenberg one that you've had with other physicists; does the discussion of that problem go back quite a ways?

Dirac:

Yes, I think it does, yes. I think most physicists take the Heisenberg point of view. Perhaps the reason why I don't take it is that I thought for a time that the one idea of non-commutative algebra would solve everything and then I found that it didn't so

I've rather gone to the other extreme of thinking that every idea will meet with only partial success.

Kuhn:

That does seem to me to be a different point. I press it only because I'd like to be sure I'm understanding you. That is, the idea of non-commutatively certainly solved a great many problems together when it came. It didn't solve all of them.

Dirac:

Well, it's left a great many. Well, I think people will get other ideas which will each solve a group of problems and leave a lot of problems unsolved.

Kuhn:

One other question out of that paper in the Scientific American, this a narrower one. You speak there quite having gotten the Klein-Gordon equation before he got the non-relativistic equation; I wondered whether he had said that to you or whether you were just taking it for granted that if he got a relativistic form that was the one he got.

Dirac:

He told me he got the relativistic form of the equation before he got the non-relativistic one. I'm not sure which relativistic form he got, but as far as I can see it must be that one, I don't see what else it could be.

Kuhn:

I certainly don't have any notion that it is another one. But the straightforward way of getting it, by simply substituting operators, he will not have had access to and I'm not sure —. He's got two different ways of deriving the Schrödinger equation in his first papers -.

Dirac:

You should ask Mrs. Schrödinger if Schrödinger's left any notes on that.

Kuhn:

We've got all the notes he left in Copenhagen now and we're going over them but there's very little from that period. I hope we will find something but I'm not optimistic about it. There are some

calculations from that period, Stark effect, this sort of thing, but nothing that looks like groping toward the wave equation at least not on our first inspection of it. I think probably one can reconstruct it and I think very likely when one does that one will come out with the Klein-Gordon equation but I'm not sure. I just wondered to what extent you'd taken this picture from things that he'd said and to what extent you'd taken it from —.

Dirac:

Well, all that he said was that he had the relativistic equation for the hydrogen atom before the non-relativistic one and he worked it out and got the wrong spectral terms.

Kuhn:

[showing Dirac] This is a photocopy which was made in Moscow of the Kapitza Club Minute Book. One of the things I particularly noticed looking at it -
-.

Dirac:

I just wonder where I left my glasses.

Kuhn:

Heisenberg's paper of 28 July, 1925, to the Kapitza Club, is given one meeting [the 94th]; the very next meeting which is 4 August, you give your first paper to the Kapitza Club. Would this early August meeting have been the first meeting at which you were a member; did you speak at the first meeting?

Dirac:

Can I just look (at it)? [Discussion of interpretation of photocopy and whether Heisenberg and Dirac did indeed speak at successive meetings].

Kuhn:

But for example, if the Kapitza Club elects new members on an academic year basis with the end of July doing this; or you could have missed the meeting.

Dirac:

I think they elected any time.

Kuhn:

They don't necessarily begin their membership by presenting a paper?

Dirac:

No, I think that would be unusual. It could have been that I had been elected previously and then missed that meeting. But I'm a bit surprised that there isn't a reference to Heisenberg if he actually talked at the meeting.

Kuhn:

Well, the standard thing appears to be for the speaker to write down the title of his talk and his own name. I rather suspect that this was pasted in, in fact it looks as though he may have had a spare passport picture with him and then he simply wrote this in and signed the picture. Because this is clearly two papers. Isn't it? Yes, this is Babcock's paper and I think both the handwriting and the subject are distinct from this and that I think is the signature that goes with [it].

Dirac:

Yes, it may be that Heisenberg gave a talk on this German subject here.

Kuhn:

You are quite clear though that you were not at that meeting.

Dirac:

I didn't hear anything about Heisenberg's matrices until I got that reprint which Fowler sent me. It wasn't a reprint, it was a copy of the proofs. I wonder if that copy of the proofs still exists? I had it in my possession for a good many years and I gave it to Fowler .

Kuhn:

I have not been able yet to get information about where Fowler's papers might be. Unless something has come in Copenhagen since I left which is possible. Those would be particularly good, of course, to find if we can.

Dirac:

It does not look like my writing ‘this meeting here. I don’t quite know why. There are some nice pictures in it from time to time.

Kuhn:

There are some quite interesting subjects also. This I must return to Sir John Cockcroft but I have the microfilm from which this was printed and we shall copy it. You give a paper just after this in, I think, December, 1925, that I wanted to ask you about. Yes, “Light Quantum Theory of Diffraction,” which came in the 15th of December, 1925.

Dirac:

I think that was just to get —. If you have a light diffracted by a grating and you assume that the momentum that each light quantum receives is quantized you get the proper diffraction law.

Kuhn:

Is this the old Duane theory in which you would assume something like an infinite grating and a periodicity over the grating spacing?

Dirac:

I think it must have been that, yes.

Kuhn:

How long in advance would one generally sign up or agree to give a paper to the Kapitza Club? This is quite an old theory and it's an odd thing for you to be speaking on at this time because by this time you would already have gotten the commutation relations, which you come right over, in fact,, and do on the 19th of January. Would this have,, been scheduled for a long time?

Dirac:

As a rule at each meeting they arranged the speaker for the next meeting, but if the lecture finished short and there was some time left over, then anybody might be asked to speak. This is a second talk. 'I was just called upon to speak just to fill in some time that was left over.

Kuhn:

You would be called upon from the chair to perform without any advance notice or would you have

volunteered that ‘if there were time you could describe ‘the following.’

Dirac:

They would first of all ask for volunteers from the chair, and maybe this was something I had just read about and thought it would be good to talk about.

Kuhn:

Were you at all impressed with that theory?

Dirac:

Well, before the discovery of quantum mechanics, I was.

Kuhn:

It would be interesting to know whether this is a date before the discovery of quantum mechanics or not — the 15th of December, 1925.

Dirac:

That is afterwards. I suppose the paper of Duane was published before

Kuhn:

Very long before, yes. I would say by this time that work was more than ten years old. There had been some further work on it by Epstein and Ehrenfest.

Dirac:

Well, the new quantum theory was pretty immature and not especially settled as a subject for talking about it. Did you see the minute book of the Delta V?

Kuhn:

Yes, and the microfilm is being made. I didn't sit down and study it but we picked out the first three volumes of the minute book to be microfilmed.

[Dirac continues looking at the Kapitza Club Minute Book] If turning pages in that reminds you of anything I would be delighted to hear about it.

Dirac:

Well, I'd have to spend some time on it I think.

Kuhn:

Why don't I leave it here and if you have a chance and are interested you might look it over.

Dirac:

Oh, this is Cockcroft's copy, is it?

Kuhn:

He will also get the microfilm back but I said I'd take it to Copenhagen and copy it there and then return it to him. But Kapitza had these made in response to requests that some sort of filming be done and it makes a handsome volume and an extremely interesting one for us. Let me take you back to Copenhagen which we'd really begun to talk about last time. Where did you live when you were in Copenhagen?

Dirac:

In a pension, ('Pension Sheck') it was called, near the town hall.

Kuhn:

Did you do any of your work there or did you work entirely at the institute?

Dirac:

I believe it was entirely at the institute. I just had a bedroom there. I don't remember having a desk. I don't think I had a desk there.

Kuhn:

Was it possible that the institute was well enough set up then so that you had a room of your own there?

Dirac:

I just can't remember whether I had a room of my own or whether I worked in the library.

Kuhn:

Can you give me any idea of what sort of a pattern your life settled into?

Dirac:

Well, I used to walk, as a rule, between my pension and the institute. Took about twenty minutes

walking along beside those lakes. Except on Sunday. On Sunday I took a long walk or excursion out into the country, very often alone, but sometimes with Bohr and sometimes with a big group.

Kuhn:

What time did you get to the institute in the morning?

Dirac:

Maybe half past nine. I don't remember so well.

Kuhn:

Would you be there then for the whole day?

Dirac:

I think I went back for my lunch to the pension. I believe I had all my meals at the pension.

Kuhn:

Then returned afterwards to the institute.

Dirac:

Yes. It meant quite a bit of walking: four times twenty minutes, a day, but that wasn't too much. I often would think over my work as I was walking around there.

Kuhn:

What would you do in the: evenings? .

Dirac:

I sometimes took walks around the town.

Kuhn:

It's a good town for that. [Knock on door] .

Dirac:

You asked me 'what I did in the evenings. One thing was to get in one of those trams and just to go to the terminus; you could go as far as you liked without paying extra - I would go to the terminus, see wherever it would take me and then walk back.

Kuhn:

And then walk back. Starting from the center of town you got some very long walks that way ...

Dirac:

Yes. Well, it would fill up the evening, maybe walking a hour or so. I got to know the town pretty well.

Kuhn:

You. were generally alone on these expeditions also?

Dirac:

Yes. I lived very much on my own.

Kuhn:

Was there anyone there whom you particularly remember seeing besides Bohr?

Dirac:

I think Klein was there. I'm not sure Gamow was, there at that time-or later.

Kuhn:

I think later. Actually the question as to who was there we can determine by other means, really more I asked this to see whether there was anyone there whom you may have interacted with more.

Dirac:

Mostly with Bohr. I'm afraid that interaction just went one way. I mostly listened to him.

Kuhn:

You told me very effectively and movingly of his concern with non- scientific problems. Do you remember the sorts of things he was worrying about in science?

Dirac:

Complementarily. I think it all centered around that.

Kuhn:

But it wasn't clearly quite complementarily yet. What form did he think of it as taking?

Dirac:

Well, I suppose it was still the correspondence Principle that was' very much in his mind. He still referred to the Correspondence Principle for some years I think after quantum mechanics really made definite equations which would replace the Correspondence Principle.

Kuhn:

In some ways don't you think he held on to the Correspondence Principle to the end?

Dirac:

I expect he did. When one gets so absorbed with one• idea one does stick to it always. Just like Einstein thought that non-Euclidian geometry would be the answer to everything.

Kuhn:

But you do feel quite strongly that quantum mechanics, when it developed, displaced the Correspondence Principle.

Dirac:

Yes.

Kuhn:

What about complementarity?

Dirac:

I don't altogether like it. In the first place it is rather indefinite. It doesn't provide you with any equations which you didn't have before and I feel that the last word hasn't been said yet on the relationship between 'waves and particles. When it has been said people's ideas of complementarity will be different.

Kuhn:

I was particularly impressed in this connection with what you say about the Uncertainty Principle, the likelihood that h is a derived constant rather than a fundamental one and that the manner in which this will react back on the Uncertainty Principle.

Dirac:

Yes.

Kuhn:

How did you feel about it at the time — the Uncertainty Principle and complementarity which come pretty much one right after the other.

Dirac:

I was dominated by the equations with non-commutative algebra; qualitative things like the Uncertainty Principle. I felt were of secondary importance.

Kuhn:

When you got the Nobel Prize, Bohr wrote to congratulate you and your answer to him is on file in Copenhagen. You there speak very wonderfully of your sense of the influence he had had on you and the likelihood that this was really a greater influence than anybody else had had. I wondered to what extent this was just politeness, but also what sort of influence you think he may have had in your work. Did it in any significant way, change its character as a result of contact with him?

Dirac:

I expect I did get a broader outlook. I did have too narrow an outlook previously when I thought action and angle variables were really the answer to everything.

Kuhn:

But that you'd already broken with by the time you got to Copenhagen. Well, possibly not.

Dirac:

Not altogether, no.

Kuhn:

Do you remember anything he said to you in the scientific realm that would have this same impact that it would come back whole, the way the remark about the two gunmen that draw their guns and then don't fire?

Dirac:

I can't at the moment, no.

Kuhn:

In this same correspondence there is, I think it's in the late twenties, after the electron wave equation

Dirac:

Correspondence between whom?

Kuhn:

Between you and Bohr.

Dirac:

Yes.

Kuhn:

In which he is again proposing violation of conservation of energy to solve some problems and you're saying you think: this will work out in due course.

Dirac:

I was rather prepared to accept it if Bohr proposed it.

Kuhn:

Were you, because on the whole you're skeptical in your responses.

Dirac:

Well, I wasn't sufficiently skeptical to propose the neutrino. I did take it as a serious possibility.

Kuhn:

I've been interested, in watching Bohr's work, by the number of times, and I'm not quite sure when it begins, but I think it begins before 1924 and before the Bohr-Kramers-Slater paper, that he suggests that the way to get Out of this one is to relinquish conservation of energy.

Dirac:

I think at that time I. was only too willing to give up, established ideas, just feeling revolutionary. I think I would have been happy if any established ideas were being knocked down.

Kuhn:

When you say this, you refer to what period?

Dirac:

This period when Bohr was proposing non-conservation of energy.

Kuhn:

But with Bohr that doesn't really specify a period because this happens over and over and over again.

Dirac:

Well, only until the neutrino was settled.

Kuhn:

But you said your own sense that you had ceased to be conservative and would now welcome a new and radical idea extends over the entire period? Now surely still.

Dirac:

Yes. Yes, for instance, one time Jordan proposed that one should go on from non-commutative multiplication to non-associative multiplication I rather liked that idea, but don't really see how to develop it mathematically. The mathematics of non-

associative multiplication seems to be pretty specialized.

Kuhn:

Did you work on the idea?

Dirac:

Yes, I don't know whether I worked on it much of the time. I have from time to time since then worked on it,

Kuhn:

Do you remember when that suggestion was made?

Dirac:

I think it was made very soon after non-commutative algebra was established. I think Jordan was the first to propose it. Have you interviewed him?

Kuhn:

No, but I shall be seeing him in June. I will see him after the middle of next month. You and he for a while somehow or other, I take it without much direct contact, again and again worked out much the same problem.

Dirac:

Who is the “he”?

Kuhn:

Jordan.

Dirac:

Yes.

Kuhn:

I think this was quite independent work and that the relative simultaneity grew out somehow from a similar approach.

Dirac:

Yes. Well, it was just that conditions were ripe for making these developments.

Kuhn:

Did they bring the two of you more together personally or more together in correspondence?

Dirac:

I don't think so, but I don't remember what correspondence we did have. I didn't know German very well at that time and I could only read it with a dictionary.

Kuhn:

You went on to Göttingen from Copenhagen.

Dirac:

Yes.

Kuhn:

Do you remember when you'd have left Copenhagen and gone to Göttingen?

Dirac:

I think it was, soon after Christmas. I remember stopping in Hamburg on the way to attend a meeting of the "Deutsche Physikalische Gesellschaft".

Kuhn:

That would date it. It doesn't date it immediately for me but it gives us a way to determine it.

Dirac:

Then I traveled with a group of students from Hamburg to Göttingen. We had a fourth-class carriage to ourselves.

Kuhn:

Who was in the group?

Dirac:

I remember (Mrs. Robertson) was one of them and she had a picture of a monkey she was showing to people. I don't know if she was married at that time. Do you know? That's also something one can check up on.

Kuhn:

By the same token let me ask, do you know when you actually got to Copenhagen?

Dirac:

It was in September. Maybe the 10th or 15th of September, I'm not quite sure.

Kuhn:

That's quite good enough. Did you find Göttingen terribly different from Copenhagen?

Dirac:

Yes. There was no one there whom I got so friendly with as Bohr.

Kuhn:

Did you see much of Born?

Dirac:

A fair amount, but not so much as I did of Bohr. I did meet some other mathematicians. I think Hubert — was he?

Kuhn:

He would have been alive, yes.

Dirac:

Maybe I went to some of his lectures. Maybe Weyl's.

Kuhn:

I think Weyl would have been in Zurich.

Dirac:

I have attended lectures of Weyl but that might have been later in Princeton.

Kuhn:

Weyl was working quite closely with Schrödinger , in the spring of 1926. It may be that he came to Gottingen in February or something of the sort but certainly Zurich was his regular base at that point.

Dirac:

I met Franck, I remember.

Kuhn:

What about Courant?

Dirac:

I met him also.

Kuhn:

That I would imagine was a more mathematical school than Copenhagen, including the fact that the physics was more mathematical.

Dirac:

Yes. I did meet more mathematicians there and I picked up some mathematics.

Kuhn:

I wanted to ask you about that because it's terribly hard for me to be sure that it isn't just the fact that it's in German and I read German differently from the way I read English, of course not as well; but the paper you submit from Göttingen in German to the Zeitschrift which is the one again on the collision problem in which you get line breadth, seems to me to read a little differently.

Dirac:

Well, my own knowledge of German was rather feeble.

Kuhn:

But outside of this the mathematics looks a little different —, I'm not sure I'm not putting this all in myself. For one thing, you start right in the beginning — and this is something I notice you do again in your later papers - you write the wave equation in the integral form which you haven't generally done before, treating the Hamiltonian as a Kernel and of course this is the one that makes it look like a matrix equation. I wondered did this have anything to do with the strong preference for matrices in Göttingen. Were you aware or oppressed or otherwise affected by it?

Dirac:

I think I must have been affected by it. But I expect that that was the most suitable way of writing the equation for that purpose.

Kuhn:

It certainly does very well. You know the famous story that is told about Born over and over again is someone saying to him after Born- Jordan
 “Elementare Quantenmechanik” comes out, ‘Why

didn't you put the Schrödinger equation in the book?" and Borns thumbing through the pages and saying, "There's the Schrödinger equation" If you look at it, yes, I guess it is but the whole approach is so different and I wondered if there's any — it's this sort of story that alerted me to this. Did people take different attitudes there toward the sort of mathematics you did?

Dirac:

Well, I would have learned a lot of mathematics and that would have influenced me.

Kuhn:

You use the word "Fourier transform" I think for the first time in that' paper. I wondered whether you'd known of Fourier transforms before.

Dirac:

Yes, I must have known them.

Kuhn:

You certainly had been using them.

Dirac:

Yes.

Kuhn:

But it could be possible, this is the sort of thing you point out you've done, is to be using them without knowing that there was a developed theory.

Dirac:

I knew about that pretty early. I probably learned about it in Bristol.

Kuhn:

It would be a natural thing to go with studying circuitry.

Dirac:

I was thinking of a mathematics course in Bristol.

Kuhn:

Do you have any particular recollections of Göttingen, the physics there? Did you run in there in any form that you directly remember to be opposition to the Schrödinger approach?

Dirac:

I think there wasn't any opposition. I remember in that meeting in Hamburg I attended quite a lot of lectures and got some understanding of what the experimenters were doing. They were still working very much on the spectroscopy, doublets, multiplet's, and the relative intensity of the spectra lines and. things like that.

Kuhn:

Was Sommerfeld at that meeting or did you ever really get to know Sommerfeld?

Dirac:

I did meet him sometimes. I can't remember whether he was at that meeting.

Kuhn:

What about Pauli?

Dirac:

I don't remember meeting Pauli there.

Kuhn:

Is he someone you saw something of later?

Dirac:

In Copenhagen. He came to Copenhagen one of the times that I was there.

Kuhn:

How did you and he get on in your view of problems?

Dirac:

I think we got along pretty well. I think he understood my point of view pretty quickly. Quicker than most people.

Kuhn:

How would you have compared Pauli and Heisenberg as physicists and with respect to their view of your work and of the field?

Dirac:

Well, maybe Pauli was a bit more sympathetic than Heisenberg. Of course, Heisenberg has really

accomplished more than Pauli by starting off the whole thing.

Kuhn:

You think of Heisenberg, as more than anyone else the key figure in these developments?

Dirac:

Yes, yes. Well, he introduced the non-commutation and even if you didn't like it—

Kuhn:

Where do you place Schrödinger?

Dirac:

I'd put him close behind Heisenberg, although in some ways Schrödinger was a greater brain power than Heisenberg because Heisenberg was helped very much by experimental evidence and Schrödinger just did it all out of his head.

Kuhn:

What about de Broglie?

Dirac:

De Broglie must have had a very good imagination.

Kuhn:

Is that a compliment? .

Dirac:

Well, I meant that as a compliment, yes. But he's perhaps not sufficiently critical. One needs to combine a strong imagination with a lot of criticism to be a good physicist.

Kuhn:

Oppenheimer indicates that when he was — the two of you were together in Göttingen he thinks you saw as much or more of him than anyone else there.

Dirac:

That is so. We sometimes went for long walks together although I had many walks alone.

Kuhn:

Was there anyone else there besides Oppenheimer with whom you might have done your walking?

Dirac:

Well, for a time there was Tamm the Russian. I met him first in Göttingen. There were a few others also. We once had an expedition in the Harz Mountains.

Kuhn:

Is your athletic interest pretty, much restricted to walking?

Dirac:

I took up rock-climbing a little later.

Kuhn:

Do you do things like tennis, golf, cricket?

Dirac:

No.

Kuhn:

Let me come back to the papers and again I think back to this first' big paper that, comes in Copenhagen which is the transformation theory paper. Reading that now, it really does two things that are sort of obviously intellectually, closely

related but very likely have separate origins, one of them being the transformation theory itself and the other one being the interpretation. I wondered how those things came together for you. It's hard to know for example how one could be looking for an interpretation, but it's hard to see how one would be looking for a transformation theory.

Dirac:

Well, I was for a long time trying to find something to replace the action and angle variables and the transformation theory of the classical Hamiltonian theory. I think I was working on that right from the beginning.

Kuhn:

Well, you thought for some time you had a replacement for the Hamiltonian theory in the uniformizing variables.

Dirac:

Yes. I wanted some equations that would correspond, that would have the same power as the transformation theory of classical theory.

Kuhn:

There's a very curious remark in a paper slightly earlier than this in which you point out that if p, q is a set of canonical variables, q -numbers, then the ones you get by applying the transformation $b \rightarrow b$ will also be canonical variables and you then say that it doesn't look as though this is much use. This is curious to me because this comes in a paper in which you cite the Born-Heisenberg-Jordan paper where they develop the powerful if not necessarily very rigorously developed transformation theory out of exactly that.

Dirac:

The perturbation theory, I believe it was.

Kuhn:

The perturbation theory, excuse me. They develop a perturbation theory but they call this thing the parallel to the classical transformation theory. That is they write $S(p, q)$ and call S the equivalent of the canonical transformation function, and then they proceed to expand it. They use it to develop a perturbation theory.

Dirac:

I expect I was a bit disappointed with the transformation from p to $sps1$ because it doesn't have the same power as the classical transformation. I would explain it now by saying that the new variables have the same eigen values as the old ones. There is no such corresponding restriction in the classical theory.

Kuhn:

You think it likely that the thing which led you to the transformation theory by one route or another was the attempt to find something that possessed the power of the old Hamiltonian technique, transformation technique, that fitted quantum mechanics.

Dirac:

Something that was equally general.

Kuhn:

And you didn't feel that the Born-Jordan-Heisenberg paper had that generality.

Dirac:

Just going from p to sps-1 ? That would not have the same generality Because it leaves eigen values invariant.

Kuhn:

Do you have any recollection how these two rather different components of the paper come together? Do you see what I mean by the two rather different components?

Dirac:

Yes. No, I don't know which of them came first, if that's what you mean. Probably the ideas came in somewhat the same way they were set down in the paper.

Kuhn:

There's one part of that paper that I find extraordinarily difficult to read with understanding. At least I'm conscious of a problem. You remember you speak of a function g of two canonical variables and r_j you then talk.—

Dirac:

I think I'd better get the paper.

Kuhn:

O.K. As a matter of fact, before I even open this, why is this the physical interpretation of quantum dynamics whereas everything else has been made mechanics before?

Dirac:

Perhaps I felt I'd gotten closer to classical dynamics. I think that was it.

Kuhn:

The place that it really enters is in physical interpretation of matrices. I think I probably have to go back. You have a function g , the two canonical variables; you point out in the introduction to the paper that you can fix classically the value of both variables; in quantum mechanics you can fix the value of only one variable; the other one will then vary and what you presume — what you're going to say now is if you fix the values of t_j you get the average value of the function $g(f,')$ for a given value

of over the whole of $r\{-$ space. Now in the discussion that follows on this, in the first place, it isn't at all clear to me that I know what you mean when you speak of having an ft -space once the values, of are fixed. And you go over here to — you make use of this sort of notation where g has now become a c -number with particular values although in some sense g is still a q -number variable.

Dirac:

Well, this is the transformation function, isn't it?

Kuhn:

Yes, here it is, but here it isn't. That is, this g is presumably still a q -number. So that the δ is a q -number, yet the limits of integration — it works out very nicely.

Dirac:

I'm consistently using the prime to denote the c -numbers and the thing without a prime to denote a q -number. And this then is just the transformation function.

Kuhn:

Here, we're all right. It's up here that because of the fact that the values of ψ can't be fixed to a numerical value with already fixed to a numerical value; yet with the ψ 's fixed t has a range of values which somehow or other now becomes c-number values for purposes of the integration yet I can't see how that enters. I follow what you do.

Dirac:

Isn't there just a probability distribution for the r ? Do I talk about a probability distribution lab that stage?

Kuhn:

You talk about the fact that you're going to come out with a probability distribution. But you're still working toward the probability distribution rather than having it. So that you seem here to be thinking of —. I understand this particularly easily if for example I suppose that the ψ 's are fixed c-numbers we're going to think of the r_i 's as ranging over c-number variables but that seems on the other hand quite in conflict with the whole approach you've

been developing. Although it may be the way you think about the problem here.

Dirac:

Well, it's just a way of leading up to the probability interpretation, isn't it?

Kuhn:

Surely it is, but it has seemed to me out of keeping with the fundamental approach that you've been developing. Because it does seem to demand that the which you insist properly cannot be pinned down once the epsilon is pinned down, still is allowed explicitly to range over the c-number domain whereas the darn thing seems to me ought to be a q-number.

Dirac:

It has a distribution of values which one can understand very well with the probability interpretation. I suppose that was just the first idea for the probability interpretation.

Kuhn:

Let me then simply point to this very last paragraph which presents issues and a statement which without being the Uncertainty Principle leads so closely into it that I just wonder if you remember at all the sort of discussions that led into that.

Dirac:

You are asking what effect this work had —.

Kuhn:

Well, I ask this against the following sort of background, that Heisenberg —. This paper is done in Copenhagen in the fall of '26, during '26-26, Heisenberg and Bohr by everyone's testimony are constant worrying and talking together about the interpretation; out of this comes a paper which seems to be drafted in February or thereabouts by Heisenberg on the Uncertainty Principle and which is full of transformation equations and of course Bohr's complementarily paper is a direct response to that. The issues raised by that last paragraph have to be central to that (whole debate). It's conceivable, but hard for me to believe, that you were uninvolved

with it. I mean this is the first place in the literature where I run head on into the problems.

Dirac:

I was no longer in Copenhagen in January and February.

Kuhn:

No, clearly I take it you were not there when those final papers emerge but there seems to have been a discussion going on the entire year on these issues.

Dirac:

Is your question what effect this paper had on that discussion?

Kuhn:

What effect it had on the discussion or whether looking at this, remembering this, and remembering Copenhagen bring you back any part of that discussion whether this would have an effect or not.

Dirac:

I expect I gave a talk about this at the Colloquium. I'm pretty sure I did. Probably I didn't present it well

enough for them to appreciate what I'd done and they still felt they had to work a good deal on it.

Kuhn:

Well, isn't there a sense in which the formulation that emerges from this is different from the one you give here in that you can have uncertainty, or probability distributions, for both of the variables, and then contain them within the region h .

Dirac:

Well, here I'm just supposing that one set of variables is well-defined and the other set.

Kuhn:

And you get averages for the other.

Dirac:

Yes. Yes, I see that this is more restricted in that way.

Kuhn:

But you don't remember delivering it or talking about it.

Dirac:

I don't, no.

Kuhn:

We'll go on to the next paper which is the theory of emission and absorption of radiation, which comes very shortly after this one.

Dirac:

Is that with the second quantization? .

Kuhn:

Yes. You don't call it second quantization. How do you feel about that name?

Dirac:

Other people introduced that terminology.

Kuhn:

Yes: I'm not quite sure when it first comes in.

Dirac:

I remember the origin of that work was just playing about with equations. I was intending to get a theory

of radiation at the time. I was Just playing about with equations and —.

Kuhn:

Really. With what equations?

Dirac:

Well, with the Schrödinger equation. I got the idea of applying the quantization to it¹ and worked out what it gave and found out it just gave the Bose statistics.

Kuhn:

Presumably you would have been fooling around with the Schrödinger equation and with an external radiation field as a perturbation.

Dirac:

Well, in the first place, just with the Schrödinger equation itself, seeing what happens when you make the wave function into a set of non-commuting quantities, quantities that don't commute with their conjugate' complexes..' I remember that was the approach that I had for that problem.

Kuhn:

But in the paper itself, you do, of course, suppose a perturbation from the start and there are really again two approaches in the paper with which you're really concerned to contrast with each other. In one of them, a particular radiation field is to be specified by variables N and 0 , which however you get directly out in terms of the Fourier components of the expansion of the field, but there's no second quantization here. In the other one, variables with the same name, N and 9 are to be taken as the most probable number of systems in various different states and you get a close parallelism between the results of these, provided you suppose that your assembly obeys the Bose-Einstein statistics.

Dirac:

I'll get the paper again.

Kuhn:

Good.

Dirac:

I suppose the main point was to show that this set of oscillators is identical with a set of Bosons. I got it first by playing about with the Schrödinger equation, in that it led to a set of oscillators, one for each point in the domain of the wave function. . . .

Kuhn:

When you say playing with it, you mean first introducing the idea of the non-commutatively —

Dirac:

Non-commutatively between V and g just to see what it would give.

Kuhn:

You're probably dead right. I have to say my initial reaction is one of skepticism because of the -a. That sounds terribly to me what one would say after this gets to be called second quantization and one really begins to think of these as non-commuting wave' functions. But in practice in this paper, the things that don't commute never really emerge yet as wave functions. This gets to be what is called second

quantization, but, there's an important sense in which I want to say it isn't yet second quantization in the paper.. The things that don't commute are initially the $a(t)$ in an expansion of a time dependent wave function. They're expansion coefficients, rather than the wave function itself. Now it's perfectly true that the wave function can be represented 'by the' sum of these. . . .

Dirac:

I think that's just to, have a discrete set of numbers instead of a continuous range• I think the only purpose of the expansion is to make the, degrees of freedom discrete.

Kuhn:

Let's just see what happens if we try letting the thing not commute with its conjugate?

Dirac:

Yes, I'm pretty sure that was the starting point of this work, and the application to radiation was just what turned out, without its being expected.

Kuhn:

Well, one's natural inclination I think in first looking at this idea of something's not commuting with its conjugate, is that a complex number after all determines its complex conjugate, and that therefore the sort of uncertainty implied in non-commutativity oughtn't exist in that case. Now, in your book later you're very careful about introducing the sense in which you can't treat a wave function as having a determined real .and having; a determined imaginary part, but nothing of that sort is introduced here. What may be implied is some sort of worry about the role of the complex nature, the complexness of the wave function or a concern of that sort.

Dirac:

I don't think I was worrying about that complexity. I was just making the wave function into q-numbers. I had by then grown very familiar with the possibility of passing from c-numbers to q-numbers.

Kuhn:

Were you concerned about the physical meaning of the undetermined phase?

Dirac:

I did worry about it for a bit, I believe.

Kuhn:

You again exploit that in the paper on quantized singularities and I wondered whether this was an older —. Do you know when you worried about that?

Dirac:

Probably from the start of the transformation theory. The phase was always something more unphysical than the amplitude of the oscillations

Kuhn:

Well, does it make any sense to suggest to you — I've got no notion whether this works out or not, but again that seems to me to be a root — that the whole indeterminateness of the phase might very well suggest non-commutativity of the wave function and its complex conjugate

Dirac:

No, I don't think so, I don't think so.

Kuhn:

No? O.K. Thinking of the wave function as a q-number doesn't to me do anything to suggest that it might not commute with its complex conjugate.

Dirac:

You have to find something it doesn't commute with. If it commutes with everything then it's no good counting it as a q-number.

Kuhn:

I think you. That's very helpful. How did this fit with what you said earlier about when —. No, I'm sorry, I'm — as soon as I remembered what you said earlier it was at the Hilbert-space point that you give up the notion of q-numbers. So you would presumably still have it here. Some of the papers that come right after yours by other people suggest that this whole process of second quantization is very much like quantization of the electromagnetic field, Here you're quantizing a -field and one may succeed in getting the electron out of this process rather as one gets the photon out of field

quantization. Did you have these ideas at all at the time

Dirac:

I think I had the idea that they could be applied to the electromagnetic field but I can't remember having definite ideas about the electron. It was always a Coulomb field which doesn't lend itself to this sort of treatment. And still doesn't,

Kuhn:

This paper is really the first one in which you do quantize the field itself — the electromagnetic field here

Dirac:

This one, yes.

Kuhn:

I'm curious to know — you said when you talked about the earlier paper, the Compton effect paper, in which you don't quantize it that probably at that point you didn't have it in mind to take a next step, which would somehow be a more full quantization, by quantizing the field. Do you have any notion at

what point you would set the idea that these quantities must be also treated as q-numbers?

Dirac:

I can't remember just when, no. It must have happened sometime intermediate between those two papers.

Kuhn:

I realize that this was a long time ago; I keep asking questions because with some of them things come back. The fact that they don't always is something I know from past experience is bound to happen. You speak at the end of this paper of conversations with Bohr as it was going on. In some papers you acknowledge help with writing and here you acknowledge discussion. What sort of a participant in discussions of this paper is Bohr likely to have been or do you remember his having been? In some ways this seems to me to be a particularly un-Bohrian paper.

Dirac:

He probably asked questions about it which I answered and he talked over certain things. I don't think it went farther than that.

Kuhn:

Do you remember at all how he reacted to it?

Dirac:

Probably with further questions.

Kuhn:

Were the sort of questions he asked with respect to a paper of this sort searching questions that helped you formulate it and helped you with your thinking about it?

Dirac:

They could be, yes. They would be searching questions, I think.

Kuhn:

I ask that only because this paper is so mathematical, so much of what happens here is in terms of the mathematics.

Dirac:

Yes.

Kuhn:

It doesn't lend itself to physical visualization easily and I —.

Dirac:

I shouldn't think Bohr would ask much about the mathematics.

Kuhn:

But then with respect to this paper what do you suppose he would ask about?

Dirac:

I can't remember. This one on the electrodynamics he might ask about the fluctuations in the field, or something like that. He might very well take

something which I hadn't thought of at all and ask what bearing it would have on that subject.

Kuhn:

I'd be terribly interested to know if you remember any questions being asked as early as this about a problem that emerges in the literature only a full year or more later. This is the question 'of whether spin has any meaning for a free electron. I think this question only gets published by Nevill Mott who gives in an appendix arguments which he attributes to Bohr and this is in a paper that comes after your electron wave equation paper. . .

Dirac:

I remember Bohr was very much concerned with that question.

Kuhn:

Was he concerned with it before your relativistic wave equation paper?

Dirac:

I believe mainly afterwards. He was wondering how real, one should think of this thing as being.

Kuhn:

Clearly that's a question that poses itself very strongly after you get spin without putting spin in. So that the question I mean to be asking is whether that really existed as a question before —.

Dirac:

I can't remember that being a question beforehand. He was always very much concerned with the possibility of measuring things, and if it turned out very difficult to measure he would wonder whether it really exists.

Kuhn:

It's quite a long time between this paper and the dispersion theory paper which is an immediate follow-up and application of this to the paper with the relativistic wave equation. Had you been working toward the relativistic wave equation all the way through? It's pretty much an intervening year.

Dirac:

I don't think I was working on it all that time.

Kuhn:

You told me before, I believe, that what you were really working toward was a linear form, one that would preserve transformation theory.

Dirac:

One that would avoid negative probabilities and preserve transformation theory.

Kuhn:

Tell me what you mean by negative probabilities. I wonder where the Klein-Gordon equation gave them.

Dirac:

Well, if you use it for just a single particle and you say the probability of the particle being in a certain place is given by $-\psi^* \psi$, there you have something which can be negative.

Kuhn:

I don't know that part of the literature as well as I should, but I haven't seen that referred to.

Dirac:

People nowadays try to get over it by saying that this quantity is the charge density rather than the probability of the particle. being at a place. If you've just got one particle present, then of course the charge density is just a measure of the probability of the particle being there.

Kuhn:

Unless you suppose the particle changes its sign. But the whole notion of getting spin into the picture was not part of that effort,

Dirac:

It wasn't a primary part of the effort.

Kuhn:

Had you realized in advance, had you conceived in advance, that one might have spin dropping out in the way it does?

Dirac:

Not very much in advance. Perhaps when I was beginning to play with this scalar product of sigma with p . I was thinking of spin then.

Kuhn:

I take it that at a fairly early point you reach the conclusion that for the relativistic wave equation you wanted a linear form, before you'd gotten to the point of playing with sigma dot p .

Dirac:

I don't know that I was as positive as that, I just felt that the quadratic form was unsuitable,

Kuhn:

The decision to take up this direction is I take it then quite unrelated to any thought at that point that you may get spin coming there.

Dirac:

Once one is certain that one has to have a linear equation, the whole thing is trivial.

Kuhn:

Was it? I take it that having had the Pauli theory, the Pauli formulation before makes it much easier to allow yourself a multi- component wave function,

Dirac:

Well, it suggests a two-component wave function; it doesn't suggest going beyond that.

Kuhn:

No, but I suppose in some sense that it's a bigger step from one to two than from two to four. It gives you an index.

Dirac:

But one doesn't see the need for these extra components when one is just trying to solve the problem of getting rid of the negative probabilities.

Kuhn:

When you started working on it as a linear equation, you say once you see the need for that the rest is trivial. Is it?

Dirac:

Well, if you see it has to be linear in the components of momentum as well as in the energy, then it's trivial. You just put in arbitrary coefficients and see what properties the coefficients have to satisfy.

Kuhn:

But did the manipulation of the coefficients and relating them, finding convenient forms for them and relating them to the Pauli spin-matrices, come pretty easily?

Dirac:

Well, finding a need for them, for these coefficients, to anti-commute, that came quite easily. That was an immediate, generalization of the anti-commutation of the three sigma's. And you always have to have their squares equal to 1. So you've got all the algebraic properties of those coefficients.

Kuhn:

And from there to the actual matrices, where you choose the representations, that came

Dirac:

Well, you just have to accept the idea that four components are necessary.

Kuhn:

But that was all comparatively quick and easy.

Dirac:

Yes.

Kuhn:

Do you remember at all other approaches that you had taken before you resolved to write the thing down in a linear form?

Dirac:

No, I don't think so.

Kuhn:

There is something you do in the first derivation of the equation that I wondered whether you had worried about at all, You set the thing up in the absence of a field and you make use of the homogeneity of space to say that the coefficients

will be independent of space and time coordinates, and then from there. you derive the anti-commutative properties and so on. You then assume that they will hold their form when you put a field on. I mean this is just taken for granted without comment, whereas it at least be open to you to worry about whether the presence of the field would not destroy the homogeneity of space which you've used in the older argument?

Dirac:

I suppose the only way to answer that was by trying an example and I told you how anxious I was when I first tried it on the hydrogen atom.

Kuhn:

But was that a particular source of anxiety? See you also talk about these new -.

Dirac:

The source of anxiety was whether it would be possible to bring in the field in such a trivial way, just following the classical analogues. It might have been a completely wrong way to bring in the field.

Kuhn:

Here is a place where you could have had a picture, a model. I mean you talk about the alphas as having to do with an internal degree of freedom of the electron. Or at least this possibility. Did internal degree of freedom have any pictorial meaning for you or did you try to give it one?

Dirac:

I didn't at that time try to figure out the properties of the alphas. I think that was first done by Schrödinger, sometime later, when he got the Zitterbewegung. My first concern then was whether this was the correct way to bring in the electromagnetic field. That was the more important problem.

Kuhn:

There the guidance from classical theory is overwhelmingly strong. You simply do the substitution that one's been doing — well, not for so long. But you thought if it would break down it would break down at that point.

Dirac:

It might very well break down there, yes.

Kuhn:

That's interesting. Because that's no longer the bothersome step,

Dirac:

Well, if you have a theory which works all right in the absence of a field, it's a very useless theory if it doesn't work with a field also. Of course, one had no idea whether it would give even an approximate right answer, because other things were so different. I suppose you don't altogether appreciate the anxiety one has when one is trying out a new idea.

Kuhn:

I think, as a matter of fact I do. I recently published a book that is still creating anxiety. Let me say immediately that I don't mean to compare the product, but the anxiety can be invoked by —.

Dirac:

You're bringing out a new idea, are you?

Kuhn:

I think. Yes. It certainly has felt new to me repeatedly in the course of —.

Dirac:

You must be pretty well confident in it if you put it in a book,

Kuhn:

Well, I 'm working in a field in which the book often has 'a role that the article will take in physics, because it's harder to cut things up into article length pieces. But ,that's a function really of difference in field. Do you remember any reactions to this paper on the electron equation. 'Was this hard for people to —?

Dirac:

No, I think they took it up very quickly. It was probably not so hard as the physical interpretation.

Kuhn:

Had people complained about the physical interpretation?

Dirac:

I don't think they complained. You were saying how people worked on it a good deal after it was published and published their own versions and if they felt the need to publish their own versions it meant they were not satisfied' with mine. While here with the electron they were all satisfied with my version.

Kuhn:

What about the negative energy problem? How did that evolve from here? Well, that was a problem right from the beginning.

Kuhn:

Your next contribution to it is two years later.

Dirac:

I felt that writing this paper on the electron was not so difficult as writing the paper on the physical interpretation. That really needed more concentrated and sustained thought to get the ideas clear. This paper on the electron was more accidental and once you got the right road it jumps out at you without

‘any effort, It didn’t need a sustained effort which the physical interpretation needed; gradually changing it from one form to a clearer form and then to a still clearer form. The negative energies were a problem right from the beginning but one just couldn’t do anything about them.

Kuhn:

Does that partly account for the fact that after this paper, you turn over to apparently quite different problems for a while. The papers that come after this are for one thing back in that category directly in taking up a problem in the literature and doing it more carefully or better. I think the next one is the quantum statistics, the basis of statistical quantum mechanics and then you do the quantum mechanics of many-electron systems.

Dirac:

Yes, I just felt I couldn’t make any progress with this. Other people were thinking about the negative energies. Oppenheimer put forward the idea that nature just arranged it in such a way that negative energies were always occupied. No, that came later. Yes, of course, one had to get the statistical ideas

clear before one could find the proper interpretation for the negative energy states.

Kuhn:

Has that got anything to do with the fact that you turned to statistics yourself? It's perfectly clear when you come back to the negative energy states, you use some of the statistics you and other people have been doing in the interim.

Dirac:

Well, it was just that I couldn't make any progress with this equation.

Kuhn:

But you went to that as another problem, not as one you thought likely to help you with this one.

Dirac:

No, another problem.

Kuhn:

The sense in which you use that material somewhat when you come back is a nice dividend but not one you had calculated on.

Dirac:

Yes, yes.

Kuhn:

At the start of the paper on ‘Professor Dirac’s group theory’, the many electron paper [Number 23] — you describe what has been happening in the field and talk then about the quite elaborate group theoretical papers that have been appearing and the problem that is presented if you have to learn all that abstract mathematics in order to do it. Then you make the remark which I think actually I probably copied -.

Dirac:

Shall I get the paper? Will that help?

Kuhn:

All right, fine. We’ll take it out of that. This is not a deep or technical point but it —. [Examining paper] The remark I’m after is the one beginning with the word “now”, [reading]”. . . group theory is just a theory of certain quantities that do not satisfy the commutative law of multiplication and should thus

form a part of quantum mechanics which is the general theory of all quantities that do not satisfy the commutative law of multiplication.”

Dirac:

I could have been more specific by saying that the group theories have certain special quantities which satisfy other special properties besides the non-commutative law of multiplication.

Kuhn:

What strikes me as odd in this statement and therefore may make it particularly illuminating —. I would have conceived of group theory as a mathematical theory and of quantum mechanics as a physical theory.

Dirac:

Well, there's a close connection in the equations. They use the same kind of equations.

Kuhn:

But in this sense I would think of quantum mechanics as the more special; in so far as they're mathematical, the extra definiteness of the quantum

mechanical equations, such as the use of the Hamiltonian, would make this the more special.

Dirac:

Well, the group theory deals with certain special quantities which have reciprocals. A good many things in quantum theory don't have reciprocals, those that have the zero eigen-value.

Kuhn:

Excuse me. I totally missed that point. This is what you had in mind in putting the point this way.

Dirac:

Probably. The group theory does deal with a special kind of non- commuting quantity.

Kuhn:

Am I right in supposing that the idea for negative energy states as holes comes pretty close to the time when you submit the paper on it? [Paper 24]

Dirac:

Yes. I don't think there would be any cause for delaying it.

Kuhn:

That paper starts by pointing out among other things that Weyl has suggested a possible relation of the proton and you then talk about the reasons you can't just say that it is a proton, the negative energy state is a proton.

Dirac:

I don't remember that remark of Weyl.

Kuhn:

Well, I'm not quite sure where it is, but you say — I think I've got it almost verbatim -e that this characteristic that the negative energy state can be looked at as an inversion of the sign of charge has led Weyl and others to relate it to the proton. Then you proceed to say that we can't simply say it is the proton because this introduces paradoxical properties that you mention.

Dirac:

I don't remember Weyl coming in at that stage.

Kuhn:

Well, he comes in a little later in the second edition of Gruppentheorie I know. But apparently whether he's come in in a way that helps form your own ideas or whether he's come in only after you've already played with the proton idea ee had you played with the proton idea significantly by yourself before you went to the hole idea?

Dirac:

No. No I hadn't gotten the connection between the proton and the electron.

Kuhn:

But you know that paper starts out by saying it's been proposed and it won't work and then —.

Dirac:

Did I say that someone had proposed it? Or did I say that someone might propose it?

Kuhn:

That paper I might have with me.

Dirac:

I've got it here. Yes I see I give a reference.

Kuhn:

Yes, that's right.

Dirac:

Well, one could just look up that reference and —

Kuhn:

There's no problem about looking up the reference and seeing what Weyl said; I'm really wondering how this suggestion struck you, and what role it had on the road to the hole theory.

Dirac:

Well, the suggestion by itself wouldn't lead to anything. There were several people I suppose that had the idea that there, was some connection but it just didn't lead anywhere. One had to get some quite different idea bringing in the statistics in order to get a workable theory.

Kuhn:

What do you mean in this case by bringing in the statistics?

Dirac:

Bringing in the Exclusion Principle.

Kuhn:

Right. Granting that that's quite a different theory, the whole manner in which it develops, it does look rather from this paper the notion of getting the proton in somehow or other had had an appeal; but there had been these problems that had to be gotten through if one was going to interpret this at all in terms of another particle.

Dirac:

.At that time I was more concerned with getting a satisfactory theory of the electron. than in bringing in the proton. I would have been quite happy have a satisfactory theory of the electron by itself.

Kuhn:

Wouldn't it have been awfully natural then to take Oppenheimer's idea and just fill all those states?

Dirac:

But then you disturb them and they don't stay filled. You can start off with them all filled but they don't stay all filled.

Kuhn:

How did you feel about this idea?

Dirac:

Well, the great worry then was the difference in the masses. At that time I thought that a positive particle with the mass of the electron just couldn't exist, otherwise experimenters would certainly have discovered it. If my theory predicted such a particle, then my theory was wrong. In order to save the theory, it would be necessary to find some cause for the extra mass of the positive particle. I was hoping that in some way the Coulomb interaction might lead to such an extra mass but I couldn't see how it could be brought about.

Kuhn:

Well, now you also compute annihilation probability in a paper just after this one and you get up to an infinite annihilation probability depending upon relative velocity.

Dirac:

Yes. That was a further difficulty.

Kuhn:

And then very quickly you get the Weyl proof that the masses must be the same.

Dirac:

Yes, I really felt that it should be the same but I didn't like to admit it to myself. Maybe that was my attitude.

Kuhn:

Well, does this mean you had pretty well decided that this was a bad idea?

Dirac:

I thought it was rather sick, yes. I believe I told you. I wouldn't say it had to be given up altogether, but it was rather sick.

Kuhn:

Somebody, and I can't remember who, said he once asked you how you felt about the theory in the period just before the positron was discovered and you said you'd, about given it up by then.

Dirac:

I didn't see any chance of making further progress.

Kuhn:

Did you still feel there was something fundamentally right about it? I mean that this notion of holes was the direction —?

Dirac:

Well, I think it was better than saying nothing at all about the negative energy states, which one would have had to do if one hadn't got this idea of the holes. It was a small step forward.

Kuhn:

In the paper on quantized-singularities [Paper 29, 1931] which is about something else, you use this theory in the introduction as an example of an approach through the mathematics, rather than through observation, and there you do say that we've got to give up the notion that it's a proton, but maybe it is a so-far unobserved particle.

Dirac:

Was I so explicit?

Kuhn:

Yes.

Dirac:

I don't remember that.

Kuhn:

I've got that one here. The later papers I have not been through before I got here. So those I've got along with me.[reading] "A hole, if there, were one, would be a new kind of particle unknown to' experimental physics, having the same mass and

opposite charge to an electron. We may call such a particle an anti-electron. We should not expect to find any of them in nature on account of their rapid rate of re-combination with electrons but if they could be produced experimentally in high vacuum, they would be quite stable and amenable to observation. An encounter between two hard gamma rays of energy, of at least half a million volts could lead to the creation simultaneously of an electron and an anti-electron, the probability of occurrence of this process being of the same order of magnitude as that of the collision of the two gamma rays on the assumption they are spheres of the same size as classical electrons. This probability is negligible, however, with the intensities of gamma rays at present available.”

Dirac:

Yes. I hadn't realized that the probability was very much greater if you just have one gamma ray hitting a nucleus, hitting a coulomb field.

Kuhn:

Does that sort of statement, so far as you know, bear any close connection, to Blackett's work? Pretty clearly it doesn't to Anderson's.

Dirac:

I don't think so. I think the experimental people go on their way, pretty well independently. But you could ask Blackett about that.

Kuhn:

Did you see anything of him?

Dirac:

Yes. He was in Cambridge at that time. I saw him quite a bit.

Kuhn:

He was, among the experimentalists, one of the people who paid relatively more attention to theory.

Dirac:

Yes, but I think he would agree that the probability of observing the results of a collision between two

gamma rays would be too small to be worth bothering about.

Kuhn:

No, I don't suggest that he set up the experiment for this reason, but there's surely a preparation in this remark for one's stopping talking about these things falling back into the source where you see a pair of divergent tracks.

Dirac:

Well, you could ask Blackett about it.

Kuhn:

In fact Heilbron has. I have not seen the transcript of that talk. It often helps, particularly when one is dealing with things this far from the event, to get reactions from more people than the one who was himself most directly involved. How did the news of the positron get to you?

Dirac:

Probably Blackett told me. Blackett would have heard of it at once from Anderson, and I expect he told me.

Kuhn:

Was it a great vindication?

Dirac:

Yes.

Kuhn:

Does that sort of event generate great immediate excitement and satisfaction?

Dirac:

I don't think it generated so much satisfaction as getting the equation to fit.

Kuhn:

Here, in a sense, you had been deprived of that satisfaction by the non-existence of the particle previously. You were able to get it back when the particle finally appeared.

Dirac:

Yes. I think I really got more satisfaction from the transformation theory.

Kuhn:

Which of these papers is your own favorite? I mean you don't have to pick just one.

Dirac:

I think the transformation theory was the best one in the sense that I was really working for the result. In other cases I got results unexpectedly, starting off with something different, with different ideas. But in the transformation theory I was working for the result.

Kuhn:

And you think the sense of working for that result goes back to the beginning of the idea of getting this into a fully Hamiltonian form.

Dirac:

I suppose that it goes back to my first reading Heisenberg's paper.

Kuhn:

And it's part of that same effort that leads you to put that in Hamiltonian form. This would seem to be a good place for us to stop.

Dirac:

Just one thing — about that Kapitza Club. I was wondering whether maybe I did speak at my first meeting and maybe they asked me to come to the meeting for the purpose of speaking on that subject. That's a possibility. There's a possibility that I got my membership by earning it, so to speak, by giving them a talk in the first place.

Kuhn:

That could perfectly well be. I've got no notion what that represents, but I wanted to show you this close juxtaposition and just see whether it brought anything out. Unfortunately, although there are annual lists of members, the members for a given year, there's no indication of when people are elected.

